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ART. I. *A case of Hydrops Articuli occurring in the Shoulder.*

Drawn up and communicated by JAMES KENT PLATT, M. D.  
Member of the College of Surgeons, London.

Plattsburg, February 3, 1821.

DEAR SIR,

The unfortunate subject of the following communication fell under my immediate care whilst I was a dresser to Mr. Travers, of St. Thomas's Hospital, in 1817; and with his permission I have thought proper, on account of the infrequency of the disease, to offer the particulars of her case for insertion in the Recorder.

Yours, respectfully,

JAMES KENT PLATT.

HENRY W. DUCACHET, M. D. &c.

JUNE 5th, 1817.—Mary Goodman, a servant woman, aged 42, was admitted into St. Thomas' Hospital. From her own account, it appears she was seized on the 29th ult. with a stiffness in the right shoulder, which she attributed to a cold; and under this impression her mistress applied, on the 2d inst. a large mustard cataplasm to the arm, extending several inches downwards from the insertion of the deltoid muscle. The application occa-

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sioned great irritation, and raised the cuticle a space the size of the hand. At present the vesications are broken, a blush of inflammation extends to the elbow, the shoulder is slightly enlarged, and the whole fore-arm and hand are œdematous. Ordered the white-wash to the excoriated surface, and pressure to be made by a roller from the extremities of the fingers to the shoulder. *Haust. magnes. sulph. è senn. p. r. n.*

June 18th.—The surface excoriated by the cataplasm has gradually healed, the œdema of the fore-arm and hand has somewhat subsided, but the swelling of the shoulder has considerably increased. *Applicetur empl. sapon.*

June 28th.—The soap plaster does not adhere. *Applicetur cerat. sapon.*

July 2d.—Complains still of difficulty in confining the applications to the shoulder, the swelling of which has now greatly increased, forming a fluctuating tumour, nine inches in its longest diameter, about the head of the *os brachii*. The fluid appears to be within the capsular ligament. When the hand is applied on either side a distinct fluctuation is perceived through the whole cavity. The general figure of the tumour is spheroidal; but the surface is less prominent on the anterior part of the joint, where the ligaments embrace most closely the head of the bone. *Infricetur unguent. hydrarg. camphorat.*

August 2d.—The ointment has produced no diminution of the swelling. *Applicetur liniment. antimon. tartarizat.*

August 16th.—The last remedy has created numerous pustules, but has had no influence over the disease. *Omittatur liniment. et applicetur plumb. subacetat. liquor dilut.*

August 27th.—The pustules have desquamated, and the sores are nearly healed; no evident alteration in the disease. A small valvular opening was made into the tumour at the most dependent part behind the joint, and an inconsiderable quantity of synovial fluid escaped, determining the nature of the affection.

September 3d.—About a pint of ropy synovial fluid tinged with blood, was evacuated by an oblique puncture in the same situation as the first. The opening was closed by adhesive plaster and a roller moderately applied.



September 4th.—Passed a restless night, being much incommoded by the tightness of the roller. Had this morning before breakfast an attack of rigors, attended with hiccough, and succeeded by heat and perspiration, and has since been seized with vomiting which still continues; skin hot, pulse accelerated, bowels open. Ordered a febrifuge mixture, the roller to be removed, and a poultice applied over the joint.

September 5th.—Somewhat better, though she had a sleepless night. Vomiting has ceased, and the pain has subsided, pulse still quick, and skin hot. Pergat.

September 8th.—The constitutional symptoms have much declined. She complains, however, of pain in the joint and inability to move the arm.

September 9th.—The tumefaction, if any difference exists, is greater than before the fluid was evacuated, and there is still a decided, though less distinct fluctuation.

September 11th.—Hiccough recurred last evening, and continued through the night; she loses strength and flesh, and is very low spirited and restless; has no appetite, and but little sleep. Her bowels have the last two days been much relaxed, and she is now taking the *mist. cretæ composit.* of the hospital. The poultice has been continued; and this morning the tumour opened anteriorly, discharging a larger quantity than before of the same kind of fluid mixed with blood.

September 16th.—The discharge has been considerable since the last report, the sides of the swelling have collapsed, the acromion is remarkably prominent, and the head of the humerus is dislocated inwards under the pectoral muscle. The capsular ligament seems to be in a state of complete disorganization, allowing the head of the humerus to be moved freely in its present situation. Ordered the arm to be kept in a horizontal posture, the poultice to be omitted; and the white-wash to be applied. *Cereviss. lbi. et vin. rubri. ℥iv. in die.*

September 19th.—Still declining in strength, complains that the wine causes diarrhœa. *Tinct. opi. LX é decoct. lini. ℥iv. pro Enemate.*

September 27th.—Sleeps badly, discharge continues profuse,

pulse 120; acromion is bare, bowels very much relaxed, for which she takes the *mistur. cretæ composit.*

September 29th.—Less disturbed in sleep, bowels less relaxed, and the discharge diminished; she is, however, evidently sinking.

September 30th.—Slept ill, is scarcely sensible, has involuntary discharges of *fæces*.

October 1st.—Died.

The disease of the joint, when examined after death, proved to be most extensive. The head of the *os brachii* was dislocated anteriorly, the tendon of the biceps having been ulcerated away. The glenoid cavity was completely destroyed; and the neck of the scapula, together with the acromion and coracoid processes, was in a state of caries. The soft parts surrounding the joint participated in the disease. The capsular ligament was totally obliterated, and the surrounding muscles formed the parietes of the sac, which encompassed the head of the *os brachii*, the inner surface of which was lined with adhesive and purulent matter.

#### REMARKS.

I presume the records of Pathology do not afford an example parallel with the above, either in regard to the extent of the disease or its situation. Richerand says, the knee is the only joint in which there has been a collection of synovia sufficient to entitle it to the name of a dropsy—"le genou qui, de toutes les jointures, est la plus sujetté aux engorgemens lymphatiques, se trouve ausi la seule qui ait jusqu'à présent offert un amas de synovie assez considérable pour mériter le nom d'hydropisie."\* The greater exposure of the knee to accidents, and the likelihood of its suffering inflammation from rheumatism, and over exercise more frequently than other joints, explain perhaps satisfactorily its greater liability to dropsy; but my present purpose is rather to inquire what practical inference may be deduced from the above case, than to discuss any collateral points.

Most surgical writers have cautioned against the danger of making openings into joints; authority exists, however, for the

\* *Nosographie Chirurgicale*, tom 3, p. 228.—1815.

practice as a dernier resort, when the distention has become great.\* Mr. Travers considered the experiment hazardous in this instance; but all attempts to produce absorption had failed, and the disease was progressing. It will be remarked, the health of the patient appeared to suffer materially from the puncture of the tumour. The present is, in my view, a strong case in confirmation of the precept, "*beware of opening into a large diseased joint.*" Though Dessault and others have removed cartilaginous bodies from the knee with impunity, the healthy state of the cavity in those instances was so different from the disorganized ligaments, synovial membrane, cartilage and bones, which we may presume to exist in *Hydrops Articul.*, that they afford no good authority for the practice in this disease.

In reviewing the case of Mary Goodman I can propose to myself no plan of treatment which could have saved her life, unless it be *amputation at the shoulder* before the disease had totally disorganized the joint and surrounding parts, and destroyed the energy of her constitution. It is true, nothing but the extremest necessity could justify such a procedure. I hesitate not, however, to venture the opinion, that in a similar conjuncture it would be the duty of a surgeon, after trying the known expedients of pressure, external irritants, &c. in vain, to give his patient *this last hope*. The question immediately suggests itself, when shall we resort to amputation? The state of the patient's constitution can alone enable a surgeon to answer this query. The same rule nevertheless obtains here as in white swellings of the knee, *amputate* before the suppuration and symptomatic fever have exhausted your patient's strength. Every experienced surgeon will affirm, that in this latter disease many lives are lost by delaying the operation too long.

#### DESCRIPTION OF THE PLATE.

FIG. 1. Represents the contour of the shoulder, in which the diameter of the tumour from before backwards is exhibited, measuring nine inches.

FIG. 2. Is a drawing of a dried preparation of the joint, preserved in the museum of Mr. Travers.

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\* "Mais lorsque tous ces remèdes ont échoué, on doit évacuer la synovie en plongeant dans la tumeur un troisquarts à hydrocèle." Richerand, 231. Ibid.



- a, The carious humeral extremity of the clavicle.
- b, The carious extremity of the acromion.
- c, The head of the humerus dislocated anteriorly.
- d, The remains of the deltoid muscle, which formed the sac.
- e, The dorsum of the scapula.
- f, The sac which contained the fluid, laid open.

N. B. I would gladly mention the name of the ingenious young gentleman, a student of Mr. Travers, who made these drawings at my request, if it had not escaped my memory.

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ART. II. *An extraordinary case of difficult Labour.* Communicated by J. BALTZELL, M. D. of Fredericktown, Maryland, in a letter to Professor R. W. Hall, of Baltimore.

I offer no apology for communicating to you an account of a most singular and distressing case of labour, which came under my care. It may perhaps serve further to illustrate some of the morbid phenomena incident to parturition. I had never read, heard, or met with a similar case, in the course of my obstetric practice; nor did it ever occur to my apprehension, that an irregular contraction of the uterus, of such a character, might frustrate its functions in the expulsion of the fœtus.

On Tuesday, the 24th of October last, in the afternoon, I was requested to visit Mrs. N. and was informed, that she had been taken with labour on the preceding Sunday evening: that the liquor amnii had passed off with the first slight irregular pains, and, as the head of the child did not advance, it was deemed expedient to call in the family physician without delay: that the presentation was right, but the pains were not sufficiently strong to propel the child, and it was apprehended that there was want of space, owing to a defect of form. Means had been applied to excite the throes of the uterus, and it was hoped, that they still might, if rendered sufficiently active, accomplish the delivery. The pains had ceased altogether, and she had had a convulsive fit, some short period before I saw her. I found her in a state of stupor, with considerable mental derangement. The attending physician informed me of the means he had employed, and that they had all proved unavailing. On examining her situation, I discovered the

os tinæ fully dilated, with the head of the child down in the basin of the pelvis, and was convinced, that the labour was not impeded by any malformation of its structure. It appeared evident to me, that, from the shock the vital powers had now received from the convulsions, it was necessary, in order to preserve her life, that the delivery should be effected with all possible despatch, and I therefore recommended the application of the forceps. In the mean time, however, as her pulse was much depressed and a great prostration of her system had taken place, it was thought proper, with a view to prevent the recurrence of convulsions, to rouse her by administering some nervine stimulants. Repeated attempts were then made with the forceps to extract the fœtus, but they were all fruitless. The head would seemingly yield a little, but by the continued application of force, the cranium would give way and slip from the blades. On examination afterwards it was discovered that the head occupied the same place as before. Being satisfied that the delivery was impracticable with instruments, I introduced my hand, with a view of turning the child and delivering by the feet. But to my utter astonishment, I reached a part so tightly enclosed with the uterus, that with the utmost exertion of my hand, I could not pass my fingers between it and the body of the child. It then also appeared to me, that, when the head seemingly yielded to the force of the forceps, it was the uterus with its contents, that was drawn down towards the pelvis, and what surprised me, after all these attempts, the contraction now extended itself in an increased degree to the os tinæ. I then suggested the abstraction of blood by copious venesection to relax the system, and to embrace the moment of its immediate effect to extract the fœtus with the forceps. This also failed. On the following morning, bleeding was resorted to a second time, but with the like failure in repeated efforts. From manual examination and combined circumstances, I inferred the case to be what is usually termed "the hourglass contraction" met with sometimes in the retention of the placenta, but, never heretofore apprehended by me, as a cause of failure in delivery. From the prostration and exhaustion of the system and the repeated recurrence of convulsions, I considered the case beyond the reach of



human means of relief, and that evening, about seventy hours after the commencement of labour, she expired. This lady was in the prime of life, of low stature, and extremely fat and corpulent, and of a healthful constitution. This was her first pregnancy, and her health was unimpaired during gestation.

The singularity of the case and its sad catastrophe induced her afflicted relatives to desire a dissection of the body to ascertain the real cause of the fatal issue. The attending physician and myself accordingly opened her body about four hours after her decease, and examined the state of the uterus. We found the deductions I had made from the manual attempts, entirely confirmed. The uterus was elongated, forming a tight case over the breach of the child, which felt hard to the touch through the external integuments; on opening these and cutting into this part, some excessively noisome effluvia escaped, evidently produced by incipient putrefaction. It was closely contracted on the body of the child, presenting inequalities correspondent to its shape. It occupied the right side of the cavity of the abdomen, pressing the intestines to the left. Near the neck and shoulders of the child, the uterus had the appearance, as if a circular band, about half an inch wide, had been forcibly drawn around it. The stricture was accompanied with a prominence on each side, forming as it were, a groove in its course around that part of the uterus. It was this which I felt in endeavouring to pass my hand over the back and shoulders of the child. This fixed and forcible contraction, as with a chord, bound fast the fœtus and presented an insurmountable barrier, defying all the efforts that were used at delivery. The fœtus lay in its natural position with its breech uppermost, the knees bent and feet drawn close to it. It was a male child, large and perfect in its structure.

From the elongated state of the uterus and the close contraction of its parietes on the body of the child, it would appear as if, in the parturient orgasm, the longitudinal fibres acted very feebly, perhaps impeded by the resistance occasioned by this stricture, if it existed, antecedent to their action, or perhaps overcome by the more powerful contraction of the circular fibres generally. In this way the energies of the uterus must have been exhausted in



fruitless efforts, and the consequences which followed were inevitable.

On a review of the case, one might be led to consider, in reference to practical inferences, whether this irregular contraction of the circular fibres of the uterus might not have been caused in some measure by a peculiarity of its organic conformation? And what would be the effects of a copious abstraction of blood and the administration of the ergot in the early stage of a labour, attended with such circumstances?

Your attention being necessarily much occupied in this department of medical science, I hope this communication will not prove unacceptable.

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ART. III. *A case of disease, in which the heart was found in the right side of the Chest.*—Communicated by ROBERT R. BARTON, M. D. of Winchester, Virginia.

I WAS requested to see, some time in August, a black girl aged ten, who had been ill for near two weeks, and had during this time taken some common remedies for fever. I found her, with a rather small quick pulse, beating 100 to the minute, dry hot skin, hacking cough, and somewhat swelled and bloated about the eyes. She was immediately bled and purged, and strong antimonial powders prescribed; in short, the whole antiphlogistic system was rigidly pursued; her breast was blistered, different expectorants exhibited, such as the compound syrup of squills, in such doses, occasionally, as to vomit; digitalis, &c. &c. without amendment. Finding this obstinacy, and the symptoms being somewhat anomalous, I determined upon closer examination, and finding her feet swelled, began to suspect hydrothorax. This induced me to try if there was fluctuation in the chest, and while examining was surprised at a strong *pulsation under the right breast*. I at first suspected aneurism, but soon found that there was *no pulsation* in the left side, or in the region of the *usual position of the heart*; I was soon convinced that the *heart* was situated further on the *right* of the

sternum, than it usually is on the left; I also felt satisfied that there was water in the chest. But whether there was here original malformation, or whether this error loci had arisen from disease, I could not say; the former I suspected, as two of the family had died I understood about the same age with nearly the same symptoms. She lingered for several weeks and died. Unfortunately I only had it in my power to open the thorax, and not to *dissect*. I found the heart pushed on the right side of the mediastinum, and considerably on the right of the sternum; the pericardium contained an ounce or two of fluid, and the left lobe of the lungs seemed to be drawn up into a knot and adhering firmly to the mediastinum, with a substance something like *calves-foot jelly*, attached to its surface. The left cavity of the thorax contained, I suppose, near a quart of water, and it was evident the left lobe of the lungs could not perform its functions from its diseased state, and indeed had not room to act. My friend Dr. Lynn witnessed the examination.

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ART. IV. *Observations on the utility of Ergot, in the management of Uterine Hæmorrhage.*—By MORRIS C. SHALLCROSS, M. D. of Delaware County.

ALL writers on the subject of midwifery agree, that when the occurrence of uterine hæmorrhage renders the delivery of the child necessary to the safety of the mother, the auxiliary means of uterine contraction is a circumstance never to be overlooked, but to be promoted by every suitable means in our power. The hæmorrhage in such cases will be checked or diminished, in proportion to the increase of effective pains. In the hæmorrhages which take place during parturition, when the placenta is properly attached to the fundus uteri, it was at first recommended to turn the child, and deliver by the feet, when other means to restrain the hæmorrhage proved ineffectual; and it continued to be the practice from the time of Parè, until M. Puzos introduced the method of rupturing the membranes, with a view to bring on the uterine action, and promote the expulsion of the child. This last method



has still continued, and is recommended by the present professor of midwifery in the university of Pennsylvania. There are two objections to this practice, which, I think, the agency of the ergot will remove:—1st. The membranes may be pierced, and the liquor amnii discharged, without exciting the uterus to act with sufficient vigour to expel the child, or to stop the hæmorrhage. 2d. If the flooding should still continue, we are deprived of the advantage of turning to deliver by the feet. The effect of ergot in exciting unremitting contraction in the uterine fibres, renders its agency peculiarly suitable to cases of uterine hæmorrhage, which are generally produced by a partial separation of the placenta. The circulation of the blood through the vessels of the uterus, is impeded during the continuance of contraction, and the reciprocal pressure of the placenta on the child and parietes of the uterus, will favour coagulation in the orifices of the bleeding vessels.

The first case which occurred to me was a woman in the forty-second year of her age, and in the commencement of the ninth month of her fifth pregnancy—the flooding commenced about day-break without any apparent cause. I saw her about three hours after it came on; the hæmorrhage had been very considerable, and was at the time I first saw her still continuing; she had not felt the child since the first discharge; her strength was much exhausted, and the remedies employed to check the hæmorrhage not having the desired effect, delivery was looked to as the only means of saving her.

The undilated state of the os uteri formed an objection to the introduction of the hand, to effect delivery by the feet, and the continuance of the hæmorrhage, with the exhausted state of the patient, made me unwilling to trust entirely to the evacuation of the waters. The plan adopted was as follows: The woman was placed in the best possible position to preserve her from syncope, and the vagina stuffed with some soft tow and part of a flag handkerchief—one scruple of ergot was then administered in substance. In fifteen minutes the pains, which had before been slight and irregular, came on with considerable energy; the substances placed in the vagina were now carefully removed; the os uteri was found about the size of half a dollar, with its edges thin



and the membranes spread tense over it. With a view to accelerate delivery, the membranes were pierced and the liquor amnii discharged; and in thirty minutes the child was born. There was no fresh hæmorrhage after the membranes were ruptured; nor in the delivery of the secundines. On the surface of the placenta were two clots of firmly coagulated blood, the size of an egg, flattened. The death of the child in this case cannot be attributed to any agency of the ergot.

The second and last case occurred on the 14th April; she was a coloured woman at the full period of gestation with her third child—she had fallen across a chair, which was followed with some hæmorrhage accompanied with a sharp pain in the side; she was immediately confined to her bed, and cold applications made to the back and region of the pubis. I saw her four hours after the accident happened; the hæmorrhage occurred at intervals but very slight; the child was distinctly felt moving in the uterus; the os uteri was soft and dilatable. The pains which came on at protracted intervals were too inefficient to be trusted, which induced me to act as in the other case. The ergot was given, and as soon as brisk contractions came on, the membranes were ruptured; the hæmorrhage disappeared; and thirty-seven minutes after the *waters* were discharged, delivery was effected without any difficulty. The child was very much exhausted, and the pulsation of the umbilical cord had nearly ceased.

The first of these cases presented considerable difficulty, in consequence of the great reduction of the patient's strength, and the undilated state of the os uteri; both these circumstances forbid a hasty attempt at turning the child; the continued discharge rendered delay hazardous.

The slight pains that existed, instead of contributing to her relief, were a real inconvenience, as they kept her uneasy, and excited alarm by the external hæmorrhage which it occasioned. The objects for which the membranes were ruptured, were: To promote delivery; to diminish the cavity of the uterus; to lessen the diameter and alter the direction of its vessels, and to make it contract firmly on the body of the child, that by its pressure it might favour the formation of coagula.

It must be recollected that in both these cases the placenta was attached at the fundus of the uterus; how this practice would succeed when the hæmorrhage takes place from the placenta being fixed over or very near to the os uteri, must be determined by further experience. Should a case of the kind fall into my hands, the flattering success which has attended these two instances of its employment, would induce me to try its efficacy again. In such a case, the child should be turned before the effect of the ergot commenced, and by that means reap the benefit of its agency to produce expulsion; no other good effect could be expected from the contraction of the uterus, which generally has the effect of increasing the hæmorrhage. With proper management, no doubt exists in my mind, of the propriety of using this medicine in almost every case of uterine hæmorrhage, where no circumstances exist that would forbid its use in other cases. Like every other efficacious medicine it may be injudiciously employed, and consequently pernicious.

There is one observation which might probably be recollected with advantage; that the membranes should be pierced when the uterine efforts become vigorous; as the suppression of the hæmorrhage and expulsion of the involucra, are principally to be expected from the force and permanency of contraction; the diminution of the cavity of the uterus, and its pressure upon the body of the child.

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To the above cases of Dr. Shallcross, I add one, which lately came under my notice, and by which the advantages of this practice were forcibly impressed on my mind.

On the 29th of February last, I was requested to visit Mrs. Jones, No. 2 Cresson's court. She was in the commencement of the ninth month of her eleventh pregnancy. I found her in a state approaching to syncope, and was informed that she had lost a very large quantity of blood per vaginam. The hæmorrhage had stopped; but before I left the house it began again. On examination, the os tinæ was found dilated about the size of a quarter of a dollar. The hæmorrhage was so copious, that un-



less something could be done to check it promptly, it was evident she must soon be exhausted. I therefore immediately plugged the vagina with a silk handkerchief, and gave her 20 grains of ergot, as soon as it could be procured. In about twenty minutes after the ergot had been given the uterus began to contract; in ten minutes more, the silk handkerchief was cautiously removed from the vagina, and, on examination it was found that the mouth of the uterus was a little more dilated, and the membranes more tense. The bleeding was very trifling at this time. The membranes were ruptured, and the waters discharged. The hæmorrhage now ceased entirely, and the parturient efforts becoming more and more violent, the patient was delivered of a dead child in about two hours after taking the ergot. The placenta with a very large and firm coagulum was expelled immediately after the child. The lochia were not more copious than common; and the woman, though much exhausted, did well.

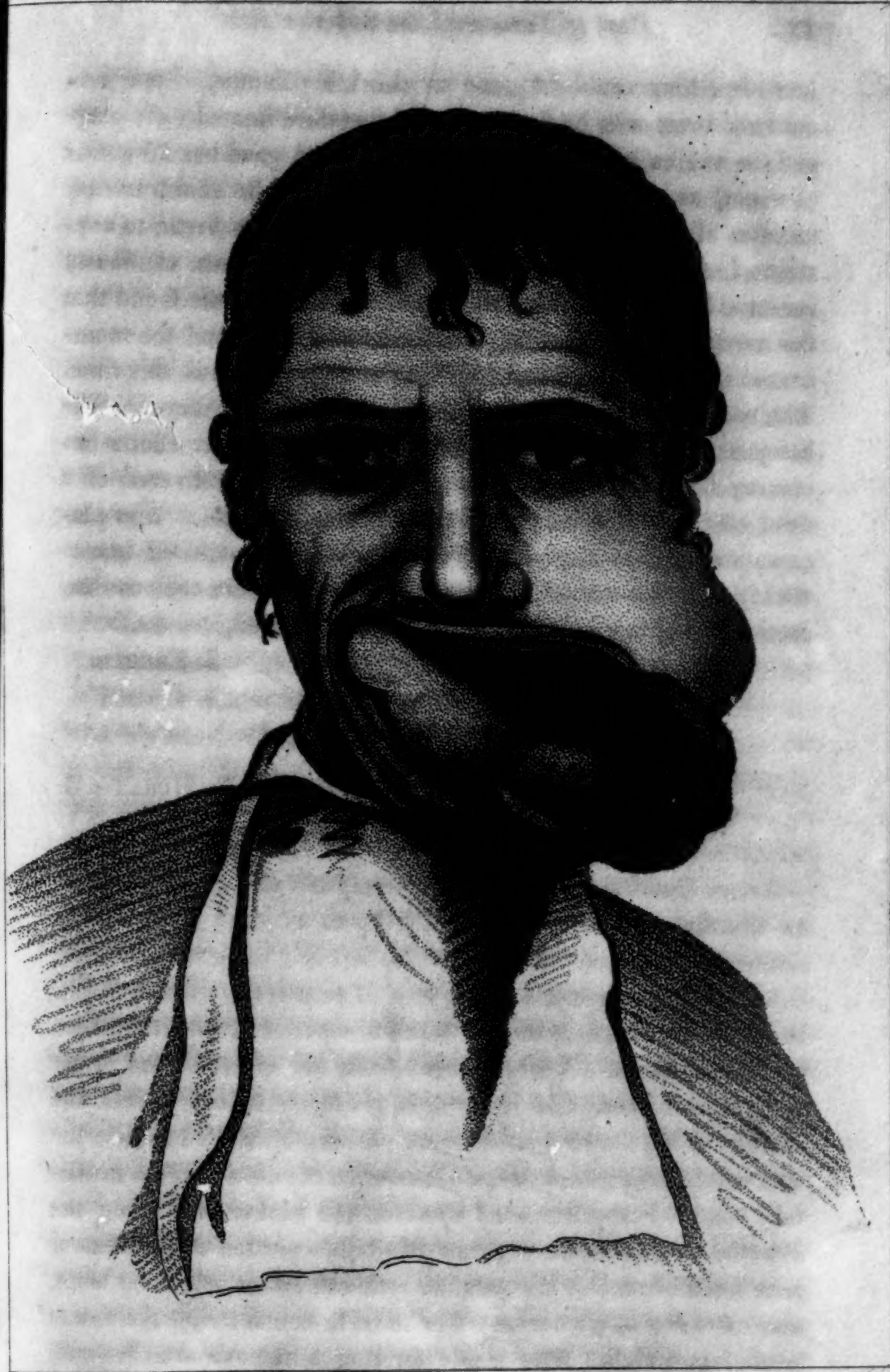
J. EBERLE.

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ART. V. *Case of Tumour of the Superior Jaw.* By HORATIO G. JAMESON, M. D. of Baltimore.

James Underwood, aged about twenty-six years, applied to me on the first day of December, 1819, on account of a tumour situated on the face. In my note book I find these observations: "James Underwood is a young man of an athletic country habit; he has a large tumour on the maxilla superior of about thirteen months duration. It commenced from the external and lower edge of the gums, and afterwards gradually extended over the middle of the above named bone, pushing upwards towards the eye, and forming a considerable tumour on the outside of the maxillary bone. It also extended upwards and backwards, along the internal surface of the superior jaw; this portion of the tumour now filled about half the palatine arch somewhat below the teeth, and extended so far backward as to be in contact with the velum pendulum palati. The whole forming a tumour which would probably weigh about two ounces. The case has been mistaken







by a medical gentleman in the country for an abscess in the antrum, and with a view of arresting the disease, he removed three of the molar teeth. But it may be remarked, that there never has been any pain in the part, nor was there any other evidence of suppuration. I believe this to be a case of tumour of the gums, so ably treated on by John Bell." I proposed an operation, to which the patient consented.

Prior to the day appointed, the patient was advised to take further advice, and went on, shortly afterwards, to Philadelphia for that purpose. He returned to Baltimore dissuaded from having any thing done. Several consultations were held in Baltimore, at which, many surgeons of the first respectability were present. All these resulted in the conclusion that nothing could be done for the patient.

About ten months after I had been first consulted, I was requested to visit Mr. Underwood again, and found him now in a most deplorable condition. After proper deliberation and consultation with my surgical friends, I agreed to engage in an operation. On Saturday, November 11, 1820, I operated on the case, and the following sketch will afford some tolerable information of the situation of the patient, and of the nature of the operation.

The mouth is stuffed in a wonderful manner with a tumour having three lobes—one growing far out of his mouth, one pushing the cheek far out over the angle of the inferior jaw, and the third extending along the roof of the mouth, and into the fauces, so as to render respiration and deglutition nearly impracticable.

The patient has not been able for ten nights to lay down at all, and for several weeks it was very seldom in his power. He sleeps in a sitting posture, but is constantly in the most imminent danger of suffocation. When he falls into a sleep he has frequently to rush violently to the window, to seek a full draught of cool air. His sleep strongly resembles a person in a slight convulsion. He has but little appetite in general: at present the small entrance through his mouth diminishes rapidly, and he is now no longer able to take what is absolutely necessary for the support of life.

His mouth is greatly distorted, the lips are thin, much extended and excoriated. That part of the tumour which is outside the



mouth is covered by brown scabs, and on its upper surface are many large vessels visible; these have bled profusely at different times, and have reduced his strength greatly. He suffers great pain through the diseased side of his face, and a soreness and distressing restlessness extends over the whole body. Incredible quantities of saliva, mucus, and purulent matter are constantly discharging from his mouth, and render his situation peculiarly distressing and unpleasant.

The base of the tumour extends from the middle of the palatine arch to the pterygoid process, and over all the space which had been occupied by the gums. The teeth are long since forced out of their sockets, and are seen sticking in different and distant parts of the tumour. The base is so very short that no very distinct view of it can now be had. But from a clear recollection of its situation last year, together with a careful examination, I was of opinion, and was joined in opinion by my friends Doctors Chapman and Harper, that the lines just mentioned included the extent of its attachments.

I had conferred often and seriously with my medical friends Doctors Mackenzie, Brevitt, Townsend, Harper and Chapman; several other medical gentlemen also, were often consulted on this distressing case, and I have much pleasure in acknowledging their friendly aid, and sanction to an operation viewed by many as totally unsafe or impracticable.

Aware of the success which attended the operation of taking up the carotid artery, by Mr. Travers, in a case of aneurism by anastomosis; and being also well aware that there was no very great danger attending the cutting off the supply of blood through the carotid, I engaged in this operation with a good deal of confidence, believing that by taking up the common carotid, I should not only obviate the risk of hæmorrhage, but thereby lessen, in a great degree, the risk of reproduction which has been hitherto so appalling in such cases.

Every thing was fully and fairly explained to the patient: the probable danger and hopes, were stated; and, after the most mature reflection, he became anxious to submit to an operation which alone afforded any possibility of recovery. Being unable to lie

down, we had him supported in the arm chair which had long been occupied, during his waking and sleeping hours of misery.

*First step of the operation.*—An incision was made one and a half inch in length along the inner edge of the sterno mastoid muscle, commencing opposite the cricoid cartilage, and carried upwards. This incision was carried through the skin, platysma muscle, and cervical fascia. Then, by the most extreme caution, the sheath of the artery, *vagus*, and internal jugular vein, was opened. In this part of the operation I found great convenience and safety, in using the common dissecting forceps. The sheath being clearly laid open, the handle of a small scalpel having a very delicate handle, was passed under the artery, and made to appear at the opposite edge of the artery; and raising the handle somewhat with the vessel upon it, a very small slit was made through the cellular structure which tied down the artery, and the handle was thus passed under the common carotid, and brought out towards the thyroid cartilage. While the vessel was elevated, a blunt hook armed with an animal ligature was passed under it as the handle of the knife was withdrawn. The ligature being now under the artery, the vessel was carefully drawn forward, so that it might be seen distinctly; and on drawing the muscle a little aside at the same time, by the handle of a scalpel, the *vagus* was plainly seen laying in the bottom of the wound. Being satisfied that nothing but the artery was within the ligature, and this confirmed by several of my friends, a single ligature of fawn skin of moderate size was tied tightly around the artery, by two simple knots. The ligature was cut close, and the wound closed by adhesive strips. This step of the operation being finished, it was instantly perceived, that the temporal artery beat feebly; and in less than one minute it ceased entirely. The patient complained of pain and confusion in his head, particularly severe about the ear.\*

\* This step of the operation was excessively perplexing, owing to the rigidity of the sterno mastoid muscles, occasioned by the long and constant use of these muscles, in supporting the head during the day and night for several weeks. The sterno mastoid presented a sharp edge, leaving an indentation or depression of the skin which extended almost to the posterior edge of this muscle. And the patient could not be put into



*Second step of the operation, or opening the cheek.*—I would have cut directly in the direction of the greater zygomatic muscle, as a matter of choice, but in consequence of the mouth's being greatly distorted, the incision began a little nearer the nose, and terminated a posture to relax these muscles, on account of the danger to which he was exposed of suffocation. Besides the prominence of the edge of the sterno mastoid muscle, and its unyielding condition, there was much difficulty encountered, from having the parts in a different relation to each other, to what they were in any dissection which I had made. Owing to these embarrassing circumstances, I had to proceed with the utmost caution; and I beg leave to admonish those who may have to perform this operation, that, under similar circumstances, they will find this a very intricate operation: and, as a mistake cannot be recalled, we cannot proceed with too much caution. Let the sheath be raised by the dissecting forceps, and deliberately examined, by scraping it clean with the handle of the knife, before it is cut. I need scarcely remark, that when we have fairly opened the sheath, and got a distinct view of the artery, we can then do no injury without the greatest carelessness.

Dr. McClellan, of Philadelphia, has written a valuable paper on the subject of taking up the arteries. I must, however, dissent, in a small degree, from what this gentleman says respecting beginning our incision, for the common carotid, opposite the cricoid cartilage, and extending it upwards two and a half inches. The safest place, for deepening the wound, in this operation, is a little below the middle of the thyroid cartilage. If we go higher, we are in danger of wounding a very considerable branch given off by the superior thyroid artery, and which passes over the sheath of the carotid, to be bestowed upon the sterno mastoid muscle. This branch from the superior thyroid artery is accompanied by a considerable thyroidal vein. It is true, there is a considerable thyroidal vein in the lower end of an incision commenced opposite the cricoid cartilage, which we are admonished by Dr. McClellan to avoid. This caution is as necessary as that requiring us to avoid the arterial branch, which I have just mentioned.—To avoid both, we must work half an inch lower than the middle of the thyroid cartilage, and with the most guarded caution: never suffering ourselves to believe, that this operation can be performed, on the living subject, safely, but by a state of mind intent on doing nothing which is not most obviously safe—disregarding the time employed. The space, ordinarily, between the thyro-sterno-mastoid artery, which I have mentioned above, and the inferior transverse thyroid vein, is about seven-eighths of an inch—by opening the cervical fascia a little below the middle of the thyroid cartilage, we will generally come upon the sheath in the most safe point—by a cautious procedure we can move a little higher, or lower, as we may find necessary; always remembering, that we are in as much danger of cutting transverse vessels in the lower as in the upper end of our incision.



about the origin of that muscle. This incision was made by a single stroke of the knife, and was conveniently performed without touching the tumour, by my holding up the lip on one side of the knife, while an assistant held up the other. The labial and facial arteries bled freely, as though no obstruction had been put upon the vessel below. I proceeded to take up the superior portion of the facial artery, but it was soon perceived that the hæmorrhagy would be of short duration. The membrane of the mouth which connects the superior lip to the gums was next divided, by one stroke of the knife, on to the nose—a second stroke cut down that portion of the buccinator muscle which is attached to the upper jaw. We had now some confirmation of the opinion which we had had of the extent of the attachments of the tumour, but there was nothing like a pedicle or cervix, all was firm and unyielding. Having now brought the tumour as much as possible into view, without having done any injury to the parotid duct, I proceeded to remove the tumour.

*Third step of the operation.*—I grasped the tumour with the left hand, then passed a scalpel having a long and narrow point through the base of the tumour, along the side of the first remaining tooth; the edge was carried upwards till it came into contact with the bone—then the knife was turned with its edge outwards so as to make the next incision at right angles with the first, and along the edge of the remaining portion of the maxillary bone—here a bold and deep incision was made the whole length of the base. The tumour being now pretty well emptied of its blood, by pressing it with a considerable degree of violence, it was more distinctly seen, that the base of the tumour extended along the palatine arch nearly to the velum pendulum palati; the tumour,

I am well aware of the importance and safety of making our first incisions sufficiently large in all operations—but there can be no good reason for carrying them beyond what is really necessary, and I am constrained to remark, that in all the cases which I have noticed the incisions were too large. One and a half inch will be found sufficient for taking up the common carotid, and there will be more chance of a speedy healing by the first intention, than were we to make our incision long, by which we run some risk of cutting branches of the external jugular vein, and thus become liable to encounter some interruption from the hæmorrhage.

however, had forced this structure deep into the throat. The incision was deepened by two or three bold strokes, and all thus extirpated except that part of the tumour which was attached to the pterygoid process—this part was got at with considerable difficulty, but was removed with very little delay.

It was now found that the internal portion of the tumour had several cartilaginous formations, intermixed with some pieces of the alveolar processes—a part only of these had been removed; those remaining were carefully sought after and removed. The body of the maxillary bone was found a good deal broken up, but not entirely removed. The incisions were not carried so high up as the bottom of the antrum; and there remained after the operation, a good deal of swelling or thickening of the parts. The tumour, though far from resembling the soft spongy structure often found in polypi of the nose, if it must have a technical name, might most properly be classed with the polypi.

*Fourth step of the operation.*—Two sutures of moderate size, formed of the common flax thread, were passed through the lip, taking care to make the threads pass to the inside of the cut surface, without leaving any space between the thread and edge of the incision, so as to prevent any puckering of the skin within. The lower one was passed through the *cherry* part of the lip, as advised by John Bell. These sutures being tied, so as to bring the cut surfaces handsomely into contact, small sewing needles were passed at the distance of every fourth inch, but no deeper than the skin. A fine thread was wrapped carefully, in the usual way, about the needles, taking care not to draw the thread so tight as to wrinkle, or unduly pinch the skin. Suitable dressings being applied, the patient arose from his chair and walked firmly across the floor.

Nothing remarkable occurred during the first week; the patient slept well some nights, he ate well, had but little fever, complained of headach, but it was not violent; the pupils of his eyes were much dilated, debility very great, very little pain of the neck; of this he complained somewhat on the fifth day after the operation.

In the second week he had severe throbbing pains through his cheek, had more fever, and some gripings, which were probably



occasioned by his indulging his appetite for solid food, from which he had been long deprived; these symptoms were greatly relieved by an occasional dose of oil. Parts cut are all soundly healed.

Third week much debility, less appetite, bowels disordered, severe suppurating pains, and considerable discharge of pus. Has been taking laudanum occasionally to considerable extent, stomach a good deal disordered towards the close of this week. This distressing affection was completely arrested by effervescing draughts.

Fourth week, recovering rapidly. Sits up at times, appetite improving, suppuration continues, and the parts look inflamed, and continue swelled.

There is still no pulsation in the temporal artery. Commenced the use of caustic—a tin tube was procured with a calibre large enough to receive a piece of caustic of the usual size, and into this tube was fitted a piece of wood, by which the caustic could be forced up the tube. This tube, furnished thus with the caustic and sliding stick, was held firmly against the bottom of the antrum, and the caustic held firmly against the part, by means of the sliding stick, for ten or fifteen minutes. A few repetitions of this opened the bottom of the antrum; and it was found that this cavity was not diseased. The bottom of the antrum being surrounded by a structure resembling, in appearance, the tumour, it was judged necessary to introduce a small piece of caustic into the antrum, with a view of procuring granulations, which might serve to fill the opening into the antrum. After passing up, and confining the caustic, by means of a bit of sponge, some sloughing was produced, and very violent pains were excited. The caustic was now discontinued internally, but outwardly it was applied every three or four days, with a hope of lessening the risk of reproduction.

From this time he has been gradually improving in health, and the swelling of the parts diminishing under the application of the vegetable caustic, applied two or three times a week. And at the present time, nearly three months since the operation, there is no appearance of a new growth. The *velum pendulum palati* is quite free, the part from which the tumour was cut is rapidly as-



suming a healthy appearance, resembling the gums in structure. His health is excellent, no pains remain; and, so far as we can foresee, there is little or no probability of a return of the disease.

#### REMARKS.

I hope this case will afford a useful precedent in the practice of surgery, in a two-fold point of view: First, there is good reason to believe, that by taking off the direct circulation and thus lessening its impetus, there will be much less likelihood of the return of tumours of the face.

Secondly, the use of a single animal ligature would seem to be the finishing stroke, as regards the application of ligatures, for the purpose of rendering an artery impervious. The labours of Scarpa, to prove that a ligature applied for this purpose might be removed safely, by the fourth day, lose all their value since the use of the animal ligature. The tape, which has been used to act as a temporary ligature, is no longer admissible. Neither does there appear to be any good reason remaining for dividing the artery in any case whatever.

If it is well ascertained that animal ligatures are absorbed by the fluids or living absorbents, (for what more could we wish?) it seems to be well ascertained, that an artery surrounded by a ligature of a suitable strength becomes impervious in three or four days; then, if the ligature which answers the purpose of obliterating the arterial channel, is gradually removed by the absorbents, we have obtained, in this important piece of surgery, all the perfection of which we can well conceive. This subject has been so fully, and so ably treated of, that it seems unnecessary to say any thing further. I will therefore conclude by merely hinting, that it would seem, that the only remaining desideratum is, to proportion our ligatures to the size of the artery, so that it may not be removed by the absorbents, before the sides of the artery are perfectly united, nor yet so large as to endanger its acting as an extraneous body, and thus preventing union by the first intention.

**ART. VI.—*On the Treatment of Denuded Nerves of the Teeth.***

By L. KOECKER, Esq. Dentist, Honorary member of the Philadelphia Medical Society, &c.

AMONG the various difficulties which the dentist has to encounter in the treatment of many complicated diseases of the mouth, are those cases, where the teeth are found in so carious a state, that in cutting away the dead and inflamed parts, with a view of preserving them by filling up the diseased cavity with some metal, the nerve of the tooth, together with its accompanying artery and vein, are laid bare, and not unfrequently wounded. When this accident arises from the unskilfulness of the operator, the patient is indeed to be pitied; for in such hands the preservation of the tooth is hardly possible, inasmuch as the requisite operation requires great skill and judgment. But the nerve of the tooth is also often laid bare, not only without any fault of the operator, but frequently as a necessary consequence of the form and condition of the disease. It may happen, when the cavity of the tooth is formed very irregularly, and the operator is thereby deceived; or it may take place when the caries and inflammation have already penetrated through the whole side of the tooth; in which case, it is better to expose the nerve, than to suffer it to remain covered with a portion of dead bony substance, by which it will continually be irritated. It may also occur, when the tooth has been previously filled up with a metal in an unskilful manner, so that the disease is thereby accelerated in its destructive progress, and a repetition of the operation rendered necessary. In such a case, therefore, it is necessary to cut away so much of the substance of the bone, in order to remove all the carious substance, and to acquire a sufficient hold to retain the metal with firmness, that the exposure of the nerve cannot well be avoided. Again, the nerve may require to be exposed, when the disease of the tooth is so situated as to require the removal of a very large portion of the tooth, in order that the diseased part may be fairly brought to view, and the cavity cleared of its dead and inflamed parts.



The remedies which dentistry supplies us with in such cases, appeared to me from the commencement of my practice, inadequate, unnatural, and contrary to reason. All the authors I have yet been able to consult on this subject, concur in recommending *the destruction of the nerve of the tooth and its investing membrane*. They advise, for this purpose, the knife, concentrated acids, but particularly the *actual cautery*.

The pain which is caused by this operation of destroying the nerve is so intense and protracted, and the appearance of it so frightful, that few patients are willing to submit to it. The bare consideration, however, of its producing a very intense pain, and its appearing very appalling to the patient, is not in itself a sufficient ground for its rejection. These obstacles may in general be overcome by the skilful and delicate operator. But there are other considerations which render the adoption of this mode of treatment not a little objectionable. The difficulties which occur in the operation itself, are by no means to be compared, as an objection to it, to the evil consequences which always come on sooner or later after the operation, and which require a particular attention.

The violent irritation which is created by this unnatural operation, in the whole nervous system, but more especially in the adjoining nerves and parts, occasions, not unfrequently, in irritable or inflammatory constitutions, in a few days after the operation, an inflammation of the whole mouth, which becomes soon concentrated upon the parts near the affected tooth, where tumefaction and suppuration take place. The pus being discharged from the swelled gums, the patient obtains some relief, but he is not cured thereby. This can now be effected only by the extraction of the tooth; an operation to which the patient soon flies for relief. In strong and firm constitutions, when this operation of destroying the nerve is performed with neatness and delicacy, these evil consequences do not show themselves so early, and generally with less violence, but occasionally also with the most frightful intensity. The tumour, after the matter is evacuated, disappears nearly altogether, and leaves nothing but a small hardness. Through this little indurated spot the matter issues, which



usually collects at the point of the root, and works its passage outwards through the thinnest side of the socket. The irritation of the dead tooth now keeps up a constant small discharge, through the opening in the indurated spot just mentioned. A portion of the matter collected at the root, is absorbed by the lymphatics, and irritating the glands in its passage through them, it destroys the healthy secretions of the mouth, which also act as a morbid irritant, both upon the other teeth of the mouth, and upon the stomach, and through this organ upon the whole system. I have known a ~~single~~ tooth treated in this manner become the cause of general disorder to the system, and of all the teeth in the mouth.

A tooth which has been deprived of its vitality by the destruction of its nerve, acts upon the parts with which it is in immediate contact, as a foreign dead body. It causes all the evil effects which are usually the consequences of a dead root of a tooth, but in an infinitely greater degree. From the moment a tooth is deprived of life, it becomes, like a dead root, a foreign and useless part in the animal economy; and causes an irritation with which the whole constitution sympathises. In the beginning, the supuration at the <sup>root</sup> of a tooth exists in the fasciculus of the nerve, and extends afterwards along the cord of the nerve. The progress of the disease opens a way for the discharge of the matter through the canal of the root. If, therefore, a tooth which has been treated after the above plan, be filled up with metal, the natural opening for the discharge of the matter is thereby obliterated, and the matter being thus confined and accumulated, works its way through the side of the socket, and produces a fistulous opening, which can only be remedied by extracting the tooth.

I conceive I have now said enough, to convince the reader how improper it is to destroy the nerve of the tooth, by such an operation, and that it is a practice that deserves to be entirely discarded from dentistry.

I will now detail my own method of operating in cases where the nerve of the tooth has become exposed; a method which I have practised during eight years past with perfect satisfaction to myself.

In treating a case of the kind under consideration, I have always held it a principal object to preserve the life of the lining membrane, and thereby to save the life of the whole tooth.

My indications for the attainment of this purpose, are:

1. To put a stop to the caries, and thereby to the irritation upon the internal membrane of the tooth.
2. To suppress the hæmorrhage, and cure the wound of the membrane, if it be wounded.
3. To protect the membrane artificially against the action of all foreign or external agents.

To obtain the first of these objects, I cut away all the unsound or dead parts of the tooth, so that every part of the carious cavity be sound, firm, and white. I give the cavity the best possible form for the reception of the metal and its firm retention. I next wash it out, with a little lock of cotton, fastened to a straight elastic probe, dipped in warm water. The cavity must be very carefully freed from the small pieces of bone that may stick to it.

If the lining membrane be not wounded, I immediately go to filling up the cavity with metal; but if there exist a wound and hæmorrhage, I resort to the treatment for the second indication, by which I endeavour to put an immediate stop to the bleeding, and to cure the wound. For this purpose, I was for some time, in the commencement of my practice, in the habit of employing mild acids and stiptics; but I did not find these applications to answer any good purpose. The first act destructively on the surrounding parts, and the second were not sufficiently certain in their operation. I therefore soon abandoned such remedies, and resorted to the actual cautery. By this application I readily effect an artificial cicatrization of the wound and a stoppage of the hæmorrhage.

I require for the operation the following apparatus: 1. A thin iron wire, fastened to an ivory handle. The extremity of this wire I file into about the thickness of the exposed surface of the nerve; and bend the wire in such a manner as to enable me to touch the exposed nerve without touching any other part of the mouth. 2. A tallow candle, with a thick wick—I direct the patient to spit out all the saliva he may have in his mouth; and then let him incline his head backwards against the head-support of



my operation chair—I put the candle into his left hand, and direct him to hold it so that the flame of it may be in a position horizontal with his mouth, and about eight inches from it; I now place myself on the right side of the patient, and holding his lips with my left hand, so that the instrument may not touch them, I again dry the cavity as perfectly as possible with a lock of cotton fastened to the point of the cauterizing wire. Having effected this, I throw away the cotton from the extremity of the wire, and heat it red-hot in the flame of the candle. With the wire thus heated, I touch the denuded part very rapidly, so that its surface cicatrizes; without however suffering it to penetrate deeply into the substance of the bone or the cavity; for this would inevitably bring on suppuration and destruction of the nerve. The nerve must be touched very quickly, and the cautery be perfectly *red* hot. It is sometimes necessary to apply it two or three times before the parts are sufficiently cauterized. When the cautery is *red* hot, it acts suddenly, and almost entirely without pain; but when it is merely hot, much pain and inflammation is generally produced.

This operation is indeed so little painful, that I have been solicited by my patients to repeat it; although before, they required no little persuasion to induce them to suffer its application. When the hæmorrhage is arrested in this way, and an artificial cicatrix formed, I then leave the further healing altogether to nature, and only caution my patient against such things as might interfere with its salutary operations.

Air is among the most injurious external agents when conjoined with moisture, upon an exposed nerve. If the nerve of the tooth is long exposed to the influence of these agents, its inflammation and consequent destruction is almost inevitable. I terminate therefore the operation, by passing to the third indication; which is, to protect the nerve against external injurious impressions, by filling up the cavity of the tooth with metal. For this purpose I wash the cavity, as before the cauterization, with warm water. I carefully remove every particle of the ashes or matter that may have been left by the cauterization, taking great care not again to wound the nerve.



The nerve which before cauterization appeared red, appears, after this operation, like a black point. I take care not to disturb this point, for if the black scar be removed, a new wound will be formed and bleeding induced. I now take a very small plate of very thinly beat lead, and lay it upon the denuded nerve and the immediately surrounding bony parts. I next fill up the whole cavity, very carefully with gold. In order that this operation may result in the desired success, it is absolutely necessary to make the proper curative applications, with the utmost degree of exactness and care, since the smallest error in this will inevitably bring on a destruction of the life of the tooth, and consequently its loss. Thus, for instance, the whole operation will prove abortive if the smallest particle of dead matter, or inflamed bony substance be suffered to remain in the cavity; such foreign dead matters left in contact with the living tooth, soon acquire corrosive qualities, and act destructively upon the contiguous parts, by irritating and inflaming. If any particles be left to remain in contact with the nerve, it is impossible that the operation can succeed properly. Even the smallest possible drop of blood left in the cavity soon becomes corrosive, and prevents the success of the operation. All kind of moisture must be removed before introducing the metals, in as much as the two contiguous metals might produce galvanic effects if there be any intervening moisture, and thus create a source of irritation and inflammation to the nerve.

When therefore the cavity is once completely cleared of the loose particles of matter and made perfectly dry, the metal should be quickly introduced, without giving it time to become moist again from the natural exhalations in the mouth. The gold should of course be fixed as firmly and compactly into the cavity as is possible, in order to prevent the insinuation of any moisture under it. In all surgical operations the ultimate success depends much upon the sanative powers of nature. After the operator has performed his duty with skill, he can only watch the efforts of nature, and assist her in her sanative operations by a due regulation of such circumstances as are calculated to influence her powers.

It may be asked why I cover the nerve with lead—I do it because, I believe that this metal has a cooling and anti-inflam-

matory effect upon the irritated nerve of the tooth; at least I conceive it possesses these qualities in a greater degree than gold. When, in the commencement of my practice I employed gold exclusively, I was but seldom successful in my labours; inflammation, pain, &c. always soon came on, and obliged me in a short time to remove the tooth entirely. Having been almost uniformly unsuccessful, whilst employing gold alone in this operation, I resorted to the use of tinfoil, as an experiment, and with this metal my success was evidently greater; though not what I desired it to be. For even when the operation succeeded with this metal, which was not often the case, it did not remain long a protection to the nerve; because on account of its thickness I could use this metal only. In all cases where the *tinfoil* is used, the tooth is only preserved for a few years; the saliva dissolves the metal, and uniting with it, acts as destructively as the cause itself.

On recollecting the cases so commonly reported of leaden bullets, even when rough and battered, having remained for years imbedded in the flesh of soldiers, I was naturally induced to resort to it in this operation. It does not occur to me that a case has been reported of any other species of metal remaining in the body for a long period without exciting inflammation and suppuration around it. My experience has ever since strengthened the opinion I drew from these facts, and I now hardly feel inclined to apologize for believing this substance less irritating to living parts than any other metal.

I have used the lead under the gold for above eight years; and I feel warranted in saying, that seven-eighths of the teeth on which I have thus operated will be preserved alive.

In the majority of cases, where the patient wishes to have a complete operation on his teeth, we find one or more *diseased* teeth, which require this treatment; and these are commonly amongst the most important, as the *incisors*, *cuspidate*, *bicuspidate*, but very seldom the *molars*. They generally occur in the upper, and rarely in the under jaw.

After the operation for the disease in question, we must constantly attend to the following circumstances: 1. The prevention



of inflammation of the nerve of the tooth. 2. If inflammation has supervened, to endeavour to prevent its terminating in suppuration.

To answer the first object; the patient must guard himself from imprudent exposure to a damp and cold air, or to any sudden transitions from heat to cold, or *vice versa*; he must also be directed to keep his teeth clean by the use of some suitable dentifrice, good brushes and warm water. If the general diathesis of the patient's constitution be inflammatory, I direct him to bathe his gums with a mixture of equal parts of strong vinegar and warm water every three or four hours. When the tooth becomes painful, I scarify the gums, and promote the bleeding by warm fomentations. After the bleeding has ended, I request him to wash with the above mixture with the addition of a little honey. If the gums are a good deal inflamed, I apply from two to six leeches, and order an antiphlogistic regimen. When there are signs of disorder or impurities in the *primæ viæ*, gentle emetics or laxatives must be resorted to. The operator must not suffer himself to be misled by the complaints of the patient, and remove the tooth, at once when the patient, through pain, desires it. In many instances the pain ends with the operation; but in other cases the tooth becomes occasionally painful during several months. As long however as the tooth possesses vitality there are hopes of a perfect cure. If the tooth, after about three months, has a natural and lively colour; is free from pain, without being insensible, or loose, then the cure may be pronounced as complete. The tooth is now secured as long as the metal remains firm in its cavity and protects the nerve against the action of external agents. If the tooth be loose, of a pale and unnatural colour—the gums red, swelled, suppurating and painful—ulcers and fistulous opening in the neighbourhood of the tooth; then we may conclude that the nerve is destroyed and the operation abortive. In this case it is best to extract the tooth.

#### CASE I.

On the 22d of May, 1818, I was consulted by Mr. C. of this city, on the subject of his teeth. He informed me, that about fourteen months previous he had put himself under the care of a

dentist, who on cutting away the carious parts of the left *cuspidate*, exposed the nerve of the tooth, *which he immediately destroyed by the application of the actual cautery*. The pain occasioned by this, was so excessive, that the dentist could not, conformably to his original intention, proceed to filling up the cavity of the tooth with gold; but was obliged to defer this part of the operation, and to request the patient to return to him again in five days. A violent inflammation and swelling of the gums supervened, which prevented the completion of the operation, at the time the patient was requested to return. He now was directed to use emollient washes for his mouth, &c. with a view of reducing the inflammation.

The swelling immediately around the tooth, became large and extremely sensible; it soon, however, broke and discharged a considerable quantity of pus. The pain was now much mitigated, and the patient expected soon to be able to bear the operation of plugging his tooth.

But the swelling did not wholly subside; the tooth was loose, and extremely tender to the touch. Four weeks after the nerve was destroyed, it was still thought improper to proceed with the operation; and the patient was comforted by his dentist with the hope, that the inflammation and swelling would not continue long, and that he would soon be in a proper state to have the operation finished.

In this hope, however, he was disappointed. The tooth remained extremely painful, the swelling of the gums increased, which again terminated in suppuration. After having thus suffered for about five months, the patient became dissatisfied with his dentist, lost all confidence in the resources of art, and determined to rely for relief on the sanative efforts of nature.

Continued pain and distress soon obliged him to abandon this resolution, and again to apply to our art for aid. He now consulted me; I found the situation of his mouth in the most painful and afflicting state. The eye tooth, the nerve of which was destroyed by the actual cautery, was loose, and so painful that the slightest pressure of the finger caused the patient to cry out. Its socket was enlarged, and the inflammation had extended to all



the alveoli of the upper jaw, from the protracted irritation of the dead tooth. The whole alveolar process was enlarged; and this enlargement was greater in proportion to its proximity to the dead tooth. These parts also were altered from their natural direction, so that the teeth adjoining the dead one, separated from it, on either side, and from being considerably pushed forwards, distorted his mouth in a very disagreeable way. The teeth that had been filed were again affected with caries, which indeed was now progressing more rapidly than it had done before they were filed. There was much tartar on all the teeth, and more especially on those which were dead, and the adjoining ones. The gums were greatly inflamed, much swelled and spongy, and upon the slightest pressure of the finger poured out a black blood, mixed with a fetid pus. The breath was very offensive; the patient was pale and emaciated, although, naturally of a very healthy and florid complexion.

The patient readily submitted to the plan of treatment I proposed to him, which consisted:

1. In removing the inflammation of the bony structure of the jaw. This I effected, in part, by the removal of the general and topical irritating causes. The causes that kept up the local irritation, were, more particularly the dead tooth, and the two diseased molares, which therefore I extracted.

2. The removal of the inflammation and suppuration of the periosteum and gums. To this end I removed the tartar carefully from the teeth, and directed the use of a mildly stimulating wash for the mouth, and the subsequent careful cleansing of the teeth.

In three months after this I found my patient so much better as to enable me to treat the individual disorders of his teeth with perfect safety; and by a few operations on these, my patient had the satisfaction of soon recovering, not only the health of his teeth and mouth, but also that of his system generally.

#### CASE II.

Mr. B——, a very respectable gentleman of this city, consulted me about his teeth on the 12th of Nov. 1817. His teeth had been operated upon some years before, rather injudiciously: many

of them had been filed, cut out and plugged, and almost all of them were still continuing to decay, and threatened with speedy destruction. All were a little covered with tartar, of which I immediately relieved them by the necessary operation.

Dec. 11th. Five teeth were again filed, and one of these cut out and plugged with gold. In the latter, the caries having penetrated into the cavity, the nerve was unavoidably exposed: it was however not wounded, and I treated it as has been stated above.

March 26th, 1818. Four teeth were filed, and four were cut out and plugged with gold. In all of them was the nerve exposed, and in one was the wounding of it unavoidable. In this latter case the nerve was cauterized and treated as directed above.

July 6th. Two teeth more were filed, and two more were plugged with gold. Both of the latter presented difficulties similar to those lately mentioned, and in both, the nerves being exposed and wounded, the same treatment was required as in March 26th.

Aug. 29th. One of the teeth thus preserved became rather painful, and formed a small tumour over the fang; and as this tooth from its situation was of no great utility, I considered it best to make no attempt for its preservation, and of course removed it, gaining thereby a greater prospect of success for the others.

Of seven teeth in the same mouth with denuded nerves, therefore, treated according to my improved method, six are now preserved alive, healthy, and perfectly useful; and these are, one upper central incisor, one cuspidatus, two bicuspides, one under bicuspidis, and one molar.



We conceive the following Essay, which was sent to one of the Editors by the Author, to be a very lucid and able statement of the arguments which may be adduced on one side of a question, which has of late excited so much interest among medical men.—ED.

ART. VII.—*An Essay on Syphilis.* By GEORGE BALLINGALL, M.D. F.R.S.E. Fellow of the Royal College of Surgeons of Edinburgh, &c.

*Satis constat ex iis, quæ supra dicta sunt, Lib. II. Cap. 8., virus venereum nulla methodo certiore, tutiore, efficaciore, profligari posse, quam hydrargyrosi.*

*Astruc De Morb. Vener. Lib. IV. Cap. 6.*

THE limits which I have prescribed to myself in the following essay, preclude me from entering at large into the history of the venereal disease, or engaging in the important discussion respecting its general treatment, which has lately occupied so large a share of professional attention. All I propose, therefore, in the following pages, is to make a few observations on the treatment of the local or primary symptoms of syphilis;\* stating, in the first place, the reasons which induce me (notwithstanding all that has been written on the opposite side of the question) to consider the mode of cure by mercury as still the most eligible.

Soon after the appearance of the venereal disease in Europe, the efficacy of mercury in promoting its cure was very generally admitted; and this remedy, which was prescribed with increasing confidence for a period of three hundred years, we have lately been called upon to abandon, for, I scarcely know what,—for confinement, rest, and water gruel—for simple dressings and decoctions of sarsaparilla:—we are called upon to relinquish one of the most powerful remedies which the *Materia Medica* contains, and to substitute one comparatively inert;—we are called upon to relinquish an article which we are in the habit of prescribing by

\* In Dr. Cullen's *Nosology*, the term syphilis appears to be restricted entirely to a constitutional affection, and hence it may be thought inaccurate to speak of the primary and secondary symptoms of syphilis; but this language is now so much sanctioned by common usage, and runs so little risk of being misunderstood, that I consider no farther apology necessary for adhering to it in the course of this essay.

grains and scruples, and to substitute one which, to use the expression of a late eminent London practitioner, ought to be given in the shape of a pudding or a pie. Under these circumstances, we may well exclaim, with a late periodical writer, "What are we now to think of experience in physic? Why was syphilis considered to be incurable before the supposed discovery of mercury as its specific? Why is the abuse of mercury in hepatitis, and other diseases, never followed by symptoms having any resemblance to those of syphilis? And why have so many practitioners been almost uniformly successful in their treatment of syphilis by mercury?"\*

In speaking of the use of mercury in the treatment of syphilis, we have the following observations by Mr. Pearson, whose experience in this disease gives him a claim to attention which few others possess. "My opportunities of administering mercury," says he, "have not extended to less than *twenty thousand cases*, and I feel myself fully authorised to assert, that it is a remedy always to be confided in under every form of lues venerea; and, where we have only this one disease to contend with, that it is a certain antidote, and as safe in its operation as any other active medicine, drawn from the vegetable or the mineral kingdom."† When treating of the sarsaparilla, the same author observes: "I have employed the sarsaparilla in powder and in decoctions in an almost infinite variety of cases, and I feel myself fully authorised to assert, that this plant has not the power of curing any one form of the lues venerea."‡ No stronger language could have been found to mark the sentiments of this eminent surgeon in favour of the superior efficacy of mercury; and when we reflect that his sentiments were thus expressed in 1807, it is not easy to believe that, within the short period which has since elapsed, such a revolution can have taken place either in the constitutions of patients, in the nature of the disease, or in the powers of the remedy, as to render them altogether inapplicable at the present day.

\* Edinburgh Medical and Surgical Journal, vol. 13, page 492.

† Pearson on the effects of various articles in the cure of lues venerea, 2d edition, page 117.

‡ Pearson, Oper. Citat. 2d edition, page 25.



When we consider farther, that, at the time Mr. Pearson wrote, he expressed the opinion of almost every well-informed member of the profession, it is matter of surprise that, within the short period of seven or eight years,\* a considerable proportion of that profession should have been led to renounce their former opinions, and to adopt a line of practice widely different from what was formerly thought necessary.

While I feel myself yet unprepared to relinquish the general use of mercury in the treatment of syphilis, and while, (to use the language of Dr. Curry) "I should deeply lament the hasty rejection of this invaluable article; because I believe, that, if impartially estimated, it will be found to be, like small-pox inoculation, though occasionally productive of inconvenience, yet the preventative of infinitely greater mischief from the spontaneous course of the disease which it is intended to mitigate;"† I am still far from being disposed to undervalue the labours of those eminent men who have recently written in favour of the non-mercurial treatment of syphilis: I fully appreciate the benefits accruing to scrophulous and phthisical patients, from the proof which has been given of the possibility of curing the disease without mercury; and I am most ready to admit, that the recent discussions upon this subject have been of infinite advantage, both to the profession and the public, by restricting the use of so powerful a remedy, which, like all others, in the same proportion that it is useful under judicious administration, is capable of doing mischief by its unnecessary, ill-timed, or injudicious employment: to recur once more to the language of Dr. Curry, "I am ready to grant, that, like antimony, opium, and every other active remedy, mercury would probably do little good, if it were not also capable of doing some harm. The knife and the caustic are unquestionably powerful, and in so far may be made dangerous instruments; but who ever blames the surgeon for employing a sharp knife or an active caustic, seeing that both the one and the other are to be directed by his eye and guided by his hand? or who would be so absurd as to

\* This refers to the date of Dr. Ferguson's paper in the *Medico Chirurgical Transactions*, vol. iv. page 1.

† Curry's *Examination of the Popular prejudices against Mercury*, page 40.

expect, that the couching needle and the scalpel, which perform such wonders in the hands of an expert oculist and dexterous lithotomist, can be employed with equal safety or success, by every clumsy or inexperienced person who may fancy himself equal to the task of using them? What these instruments are, then, with respect to the surgeon, I contend, and hope to prove, that mercury is, under the management of a judicious physician, capable of doing, with a speed which is often indispensable, and for the most part with perfect safety, what no other means, hitherto known, can at all effect.”\*

In professing myself an adherent to the mercurial treatment of syphilis, it becomes necessary to acknowledge, that it is the only mode of treatment of which I have had any extensive experience; but that experience appears to me, when taken in conjunction with the opinions of many eminent writers, so conclusive in favour of this remedy, that I feel no common degree of reluctance to believe that I have hitherto been adopting a mode of practice which was either unnecessary or improper.

Upon referring to a register of sick in my possession, which was kept at Masulipatam in the East Indies, from the 15th of March, 1810, to the 17th of February, 1811, I find that, out of a detachment of the 2d battalion of the royals, consisting of about *five hundred* men, *eighty-six* venereal cases were admitted into hospital; that these were, upon an average, *twenty-two* days each under treatment; and that not more than *seven* secondary cases could possibly have occurred.†

This is the only period of my experience in the treatment of syphilis, of which any documents in my possession enable me to speak with precision: but when I recollect that nothing peculiar was observed, during that period, either in the appearance of the disease, in the progress of the cures, or in the number of secondary cases occurring, I cannot but look on the above with some degree

\* Curry on the Popular Prejudices against Mercury, page 22.

† The proportion of venereal cases which occurred at Masulipatam was greatly below what I was accustomed to meet with in India, owing to the vigilance and activity of Mr. Annesley, the garrison surgeon, who superintended the Lock Hospital.



of confidence as a tolerably correct estimate of the result of all my observations on this point, and the estimate must necessarily be rather an unfavourable one, from my having included, under the head of secondary cases, all those whose names occur a second time in the register, although of these it is probable some were re-admitted with recent infections instead of secondary symptoms. When I consider again, that, for many years of my life, I was in the habit of seeing at least from *ten* to *twenty* venereal cases daily; that these cases were almost uniformly treated with mercury; that the cures were as speedy, and the relapses as few as I have stated them to be at Masulipatam; and that, in the whole course of my observation, I have seen only *one* man die of this disease, I must necessarily look upon mercury as more uniformly successful in the cure of syphilis than any other remedy in any other disease with which I am acquainted.

Although the recent discussions upon this subject have in some measure taught us to look upon syphilis as a progressive disease, which in many cases will exhibit cutaneous eruptions, and other secondary symptoms, whatever mode of treatment may be adopted; and although we may thus consider ourselves relieved from a load of responsibility which was formerly thought to lie heavy on the shoulders of the profession; I find that patients are not yet by any means prepared to go along with us in shaking off this burden, but are still almost uniformly inclined to believe, that, by a judicious treatment of the primary symptoms, the disease may be cut short in its progress, and the occurrence of a secondary affection altogether superseded;\* and if it shall eventually appear that secondary symptoms are more rare where mercury has been employed in the cure of the primary ulcerations, which some statements render probable, no apprehension of the injurious effects of this medicine on the constitution (which it has lately been the fashion to exaggerate) should deter us from its employment: For when I reflect upon some thousand cases, both of syphilis and liver disease, in which I have employed mercury, with a hand perhaps

\* See some observations on this subject in the *Edinburgh Medical Journal*, No. 64, p. 456.

too unsparing, and when I think of the health which many of my patients have afterwards enjoyed, I cannot believe that there is any great proportion of human constitutions upon which this medicine exerts the deleterious effects which have lately been ascribed to it.—“Men may amuse themselves by declaiming against mercury, as an uncertain remedy; they may utter querulous details of its baneful effects, and relate tragical stories of its malignant influence on the bodies and minds of those who use it; but surely all this turbulent eloquence may be directed with equal advantage, not only against every potent article of the *materia medica*, but against the very aliment by which we are sustained.”\*

In support of my opinion of the safety of mercury, when employed with a tolerable share of judgment, many strong passages might be selected from the writings of Mr. Benjamin Bell, Mr. Pearson, Dr. Curry, Dr. Watt, and others; but the following passages from two writers who have done much to correct the abuses formerly existing in the employment of this medicine, will probably be admitted as less objectionable evidence: Mr. Carmichael, in his reply to the review of his *Essay on Venereal Diseases*, says, “I beg to observe, that I have not, nor do I believe that any other person has, witnessed ulcers on the skin and throat, and nodes on the bones, from the exhibition of the most extensive courses of mercury in any other than venereal diseases, nor even an eruption except the well-known mercurial eczema;”† and Mr. Matthias, in summing up his observations on the mercurial disease, observes, “when this mineral is administered with prudential reserve, and with discreet knowledge, its effects are *blessed, safe, efficacious and permanent*.”‡

Having thus finished my preliminary remarks on the use of mercury in Syphilis, I proceed to the more immediate business of this Essay, the consideration of the primary symptoms of the disease.

\* Pearson, *Oper. Citat.* page 116.

† *Edinburgh Medical and Surgical Journal*, vol. xi. p. 436.

‡ Matthias on the Mercurial Disease, third edition, p. 256.



## CHANCRES.

The much-admired definition of chancre by Mr. Hunter, is only applicable to a very limited number of the ulcerations on the genitals which now come before us in the common course of practice, in so much that Mr. Carmichael observes, "this disease, as described by Hunter, has diminished in so extraordinary a degree in this country, that, strange to say, I have from that period met with only one case of true chancre."\*

To Mr. Carmichael we are under great obligations for his description of the phagedenic and sloughing ulcers of the genitals, and for his observations on the injurious effects of mercury in their treatment: but the practical utility of his other subdivisions of primary venereal ulcers, does not, I confess, appear to me by any means so obvious; and, if in any instance, we have introduced too much refinement into the diagnosis of disease, it is, I apprehend, in the case before us: in affections of the internal organs, the mistaking disease of one viscus for that of another may, in many cases, be productive of the most serious evils; but in the case of external ulcers, cognizable by the sight, and by the touch, seated upon the same parts of the body, occupying the same structure, originating in the same way, and so much alike that they are liable to be confounded one with another, even by a cautious observer, I will venture to assert, that the same or similar remedies are likely to prove beneficial, and that, for all practical purposes, the necessity of a very minute diagnosis is done away.

As the characteristic of the Hunterian chancre, "the circumscribed hardness of the edge and base," is admitted to vary in degree, and as this hardness may be shaded down until it becomes nearly undistinguishable, I think it advisable not to confine ourselves too rigidly to this definition in deciding upon the mode of cure; particularly if we are to exclude from the beneficial operation of mercury all ulcerations of the genitals which do not possess the Hunterian characters of chancre: for my experience convinces me that the cure of many of these ulcerations will be expe-

\* Viz. from the period at which the publications of Messrs. Guthrie and Rose fell into his hands.

dited by mercury, when a circumscribed hardness of the edge and base does not exist in any remarkable degree.

From the above considerations, I am inclined to follow Mr. Benjamin Bell in extending the appellation of chancre to sores on the genitals which offer a considerable variety in appearance: keeping in view the following observations which are applicable to a large proportion of these sores. "Chancres appear occasionally over all the external parts of generation, and in some instances even on the contiguous parts. I have known them form over the whole scrotum, on all parts of the penis, and even on the lower region of the abdomen, immediately above the pubes.

"In some cases there is only one chancre, but for the most part we meet with two, three, or even more; nay, in some instances, they cover almost the whole prepuce. In this case they run into one another, none of them are distinct, and the whole, when thus connected, give the appearance of a foul ulcer with hard edges, an unequal surface, and discharging a foetid ill-conditioned matter.

"A real chancre is seldom so large at first as the base of a split pea; the edges of the sore are elevated, somewhat hard, and painful; but although this is very commonly the case, yet, in a few instances, it is so much otherwise, that, instead of a small circumscribed sore, we meet with a slight superficial ulceration, not attended either with pain or hardness, and which, by the consequences that ensue, we find to be venereal."\*

With regard to the mode of treatment to be adopted in chancres, I have already at some length given my reasons for considering the employment of mercury as very generally advisable: at the same time, I may observe, that where no other symptom exists, the mercurial course required for the cure of chancres is neither severe nor protracted; in many instances a slight degree of ptyalism, kept up for three weeks or a month, will prove sufficient, particularly if the sores cicatrize under the use of local applications within this period. As to the best mode of introducing the

\* Bell on Gonnorrhœa Virulenta and Lues Venerea, 2d edition, vol. ii. pages 15, 16, and 19.



mercury into the system, different opinions exist amongst the best informed practitioners; and this is perhaps a strong argument for considering it a matter of minor importance. Circumstances may render it highly inconvenient for a patient to adopt the mode of cure by friction, and in such cases we may have recourse to the common blue pill: this again will be found in some constitutions to irritate the stomach and bowels, to produce griping, purging, and tenesmus; in which cases it will become proper to substitute friction, or to combine opium with the internal use of mercury. Confinement to the house is in all cases advisable, and should be urged even at the risk of putting the patient to much personal inconvenience; but as we know that many patients are every day undergoing courses of mercury for the cure of syphilis, who are under the necessity of concealing their complaints, and of following their usual occupations, we cannot consider a rigid confinement within doors so absolutely indispensable as some have represented it.

It is in all cases of chancre a desirable object to heal the ulcerations with the least possible delay; and in order to accelerate this, caustic applications have been very extensively and very successfully employed. My observations upon this point, however, confirm very decidedly the statements made by Mr. Bell, as to the risk of inducing buboes by an early application of caustic, and the propriety of deferring this and other stimulating applications, until the mercury has affected the system. At page 328 of the work already quoted, Mr. Bell states, that out of twenty cases of chancre occurring in the common routine of practice, ten were treated by an immediate and effectual application of lunar caustic; while, of the other ten, five were treated with blue ointment, and five with common wax ointment. Of the ten treated by the application of caustic, no less than eight became affected with buboes, while only one bubo occurred in all the others. Mr. Bell conceiving also that buboes appeared less frequent from the application of caustic, where mercury had been previously administered, put this likewise to the test of experiment in a way to which I think little objection can be made. "Of forty-eight patients with chancres in an incipient state, and exactly as they

occurred in practice, one half was treated in the manner that I have mentioned, by destroying the chancres with caustic immediately on my being desired to see them; while all the others were put under mercury for eight or ten days before caustic was used. In every other circumstance the method of treatment was the same. The difference, however, surprised me greatly. Of the twenty-four treated with the immediate application of caustic, *twenty* were seized with buboes, while only *three* buboes occurred in an equal number to whom mercury had been previously administered.”\*

These statements of Mr. Bell's have all along had much influence in deciding my practice in the treatment of chancres; and I can truly assert that all my observations tend to confirm his conclusions on this point.

Until the system becomes impregnated with the mercury, I would advise that the chancres be simply kept clean, by frequent washing and dressing with dry lint; and so soon as the mercurial fetor is perceived in the breath, and a slight degree of ptyalism commences, the sores are to be touched with the lunar caustic, or dressed with an ointment containing a proportion of the oxydum hydrargyri rubrum, or sub-acetas cupri; the latter I have found to be an excellent application to ulcers of this kind, and am even inclined to think it more generally useful than the red precipitate. The lotio hydrargyri oxymuriatis flava of the Pharmacopœia Chirurgica, is an application frequently found useful; but I recommend, with more confidence, from more extensive experience, the lotio hydrargyri submuriatis nigra, or black wash, of the same Pharmacopœia, which is, perhaps, of all other applications with which we are acquainted, the most extensively useful to venereal chancres. The various degrees of indolence or irritability with which chancres are accompanied, will render the more or less stimulating of these applications preferable, and the same circumstance will suggest the propriety of occasionally varying the proportions of their component parts; it will also be observed in the treatment of these sores, that an application which in the first in-

\* Bell on Gonorrhœa Virulenta and Lues Venerea, 2d edition, vol. ii. page 329.



stance appears to be highly useful, soon loses its effect, from the parts becoming habituated to the same stimulus; and whenever the healing process seems to be at a stand, under one remedy, it is, in general, a sufficient ground for changing the application.

#### BUBOES.

“A venereal bubo is a painful swelling of a lymphatic gland, produced by absorption of the venereal virus.”\* These swellings occurring in the glands of the groin, are, in a great proportion of cases, so obviously the offspring of chancres, situated on the penis, and in some cases so easily to be traced to this source, through the medium of an inflamed lymphatic, as to have rendered it questionable whether they ever originate without the intervention of chancre; were we to subscribe to this opinion it would do away much of the perplexity which is frequently experienced in deciding upon the nature of glandular swellings in this situation; it is now, however, well established by repeated observation, that venereal buboes, capable of producing constitutional symptoms, do frequently originate without the previous appearance of chancre, and we are thus deprived of the important diagnostic symptom which would otherwise be afforded.

In most cases, the first indication of the formation of a bubo is a sense of weariness, stiffness, and pain in the groin, sometimes accompanied with numbness in the thigh of the affected side. Upon applying the fingers to the part, one or more of the inguinal glands are found to be somewhat swollen; and if a chancre exist only on one side of the penis, the swelling will be found to occupy the corresponding groin. These tumours are, for the most part, at first readily moveable under the skin, but as the swelling increases, the integuments naturally become more tense, and the tumours more stationary and more painful. Great variety exists in the violence of the inflammatory symptoms with which buboes are accompanied, and the rapidity with which they advance towards suppuration; although in most instances the tendency to this termination is strong, and requires the most decided antiphlo-

\* Bell, *Oper. Citat.* 2d edition, vol. ii. p. 26.

gistic means to counteract it, particularly in young and vigorous subjects, yet we meet frequently in phlegmatic, in irritable, and scrophulous habits, with a description of bubo increasing slowly, attended with little pain, redness, or heat, and showing little tendency to suppuration.

The only tumours with which the venereal bubo is likely to be confounded, are scrophulous enlargements of the glands of the groin; tumours of the same glands originating from gonorrhœa, denominated sympathetic buboes; and tumours originating from ulcerations or cutaneous eruptions on the lower extremities. Lumbar abscesses and herniæ, both inguinal and femoral, are also said to have been mistaken for venereal buboes; but this is a mistake so obviously the result of ignorance or inattention, that I do not conceive any practitioner of common judgment likely to fall into it. Between the true venereal and the scrophulous bubo, I regret to say, that we do not possess any adequate means of distinction: the former, however, is, in general, confined to one gland, is exceedingly painful to the touch, and this pain is said to undergo an exacerbation during the night: the tumour is, in general, rapid in its progress, and attended with considerable inflammation of the integuments. In scrophulous affections again, tumours are frequently distinguishable in more than one of the inguinal glands, and occasionally also in other parts of the body; these tumours are much less painful, they are more moveable under the integuments, their progress is slower, and attended with less inflammation on the surface; at the same time, they frequently acquire a much larger size than what the venereal bubo reaches. In cases of gonorrhœa, where the inflammation runs high, and where the habit is irritable, swellings frequently occur in the glands of the groin which are extremely difficult to be distinguished from syphilitic buboes: they are chiefly to be known by the absence of chancres, and by observing whether or not they correspond in their origin, progress, and decline, with the extent of the urethral inflammation. Ulcerations and cutaneous eruptions on the inferior extremities sometimes give rise to enlargements of the glands of the groin, which are to be distinguished from venereal buboes, by our knowing that such ulcerations exist, by the total



absence of other venereal symptoms, and, by observing that these tumours are frequently seated lower than venereal buboes, and more directly on the fore part of the thigh.

Whenever our assistance in the treatment of venereal buboes is required, at a period sufficiently early to render their cure by resolution practicable, the propriety of attempting the discussion of every tumour of this kind, is now so generally admitted, that it appears to me unnecessary to adduce arguments in favour of this practice. No time should be lost in putting the patient under a course of mercury, which will require to be continued for a longer period than is necessary for the cure of chancres; a month or six weeks will, however, prove in a large proportion of cases a sufficient time to keep up the mercurial affection of the system; and the mode of introducing the medicine by friction on the thighs, has been thought by some to deserve a decided preference in many cases of bubo, from its being thus made to pass through the diseased gland. Every part of the antiphlogistic regimen is to be combined with the use of mercury, and the assiduous employment of every local means to promote the dispersion of the tumour.

General blood-letting is a measure not often proposed in cases of this kind, and would probably very often be objected to on the part of the patient; there is no doubt, however, that when the inflammation is violent, in plethoric habits; and particularly, when, (as frequently happens) there is a fiery erysipelatous appearance on the surface of the tumour, general blood-letting is the only means which can effectually avert suppuration. The abstraction of blood by means of leeches applied to the swelling is one of the most eligible and powerful means we possess, of alleviating the pain, tension, and other inflammatory symptoms attendant on these tumours; at the same time the application of sedative and astringent lotions, particularly solutions of the acetate of lead, is recommended by long and repeated experience.

In speaking of the diagnosis between venereal buboes and scrophulous swellings of the glands, I have pointed out the torpid and indolent nature of the latter as their chief characteristic; and in practice we must be prepared to meet with tumours of a mixed nature, evidently originating from a venereal infection, while in

their progress they seem more akin to the scrophulous bubo, remaining for days and sometimes for weeks quite stationary, without showing a decided tendency either to resolution or suppuration. This is one of the most tantalising occurrences we meet with, the patient at every visit evincing a very natural anxiety to know what is to be the issue of his case, and urging his medical attendant to say whether or not his bubo will suppurate. In such cases I am happy to think that we have a remedy in our power, which will, in a majority of instances, procure the discussion of the swellings, and in all of them will certainly expedite their termination either in resolution or suppuration—I mean the application of blisters to the surface of the tumours. This is a practice by no means so general as it ought to be, and to some I know that it is altogether new. Even since I began this Essay, I have learned from the conversation of some of my medical friends, who have had considerable experience in the treatment of syphilitic cases, that the application of blisters to venereal buboes is a practice of which they had no knowledge. The practice in question is one which I have been in the habit of using very extensively ever since I entered the army, now nearly fifteen years ago, and it may be supposed I am not the less inclined to persevere in it from finding it recently recommended in the following terms by a gentleman of Mr. Carmichael's talents and experience. "The buboes in this form of venereal disease, are often remarkably hard and indolent, evincing neither a tendency to disperse nor to suppurate. In such cases, the greatest advantage may be derived from the repeated application of blisters to the indurated bubo, which soon either cause the dispersion or the suppuration of the tumour, and thus free the patient from a troublesome symptom which might otherwise continue many months to torment him."\*

When our efforts have failed in averting suppuration, and when it becomes evident that this process must take place, it is to be promoted by the assiduous use of warm fomentations, and emollient cataplasms; the latter to be applied as warm as the patient can bear, and to be frequently renewed.

\* Carmichael's Observations on the Symptoms and Specific Distinctions of Venereal Diseases. London, 1818, 8vo. page 21.



For the purpose of opening venereal buboes, (which in general ought to be done as soon as a fluctuation is distinctly perceptible,) a common or an abscess lancet, entered at the most dependent point of the tumour, and carried up through the centre of it, is in general the most eligible instrument; and nothing should induce us to be too sparing in the extent of the opening, which frequently leads to the formation of sinuses requiring renewed operations, and unnecessarily protracting the sufferings of the patient. Any hæmorrhage which occurs from an opening of this kind, is in general rather beneficial than otherwise; but should it in any case prove troublesome, it may be restrained by a piece of lint inserted between the lips of the wound, and secured by a compress and tight bandage; and in no case where buboes are of any considerable size, would I permit the lips of the wound to come into contact, as they sometimes adhere very speedily, and matter again accumulates within the tumour, demanding a new opening for its evacuation. When sinuses form either from a bad habit, from the tumour having originally a very extensive base, from the opening being too long delayed or too limited in extent, they are to be immediately laid open throughout their whole course, and the sores treated according to the common rules of surgery.

In the opening of venereal buboes, some have given a preference to the use of caustic, alleging that by destroying a portion of the distended and superabundant integuments, it accelerates the cure. I have scarcely, however, any personal experience of this practice, and have generally found that, where the opening with the lancet was sufficiently extensive, and the dressings admitted to the bottom of the wound, any superabundant portion of integuments which was found overhanging the surface of the sore was speedily reduced by suppuration, and did not often prove an obstacle to the cure.

In cases where buboes of an extensive size advance to suppuration, and where the integuments appear firm and little discoloured on the surface, a small seton, passed through the base of the tumour, will be found a good mode of opening it.

In the following case, having failed to procure a passage for an

eyed probe, containing a seton, a cure was accomplished, rather unexpectedly, by the use of an astringent solution.

In the month of October last, a gentleman applied to me for the cure of an open bubo, which, in consequence of what I considered very inefficient practice, had been allowed to come to suppuration. While endeavouring to heal the ulceration, a small tumour, resembling a common boil, formed above, and nearly an inch and half distant from the original sore; in a few days, this tumour was found to contain matter, and on pressing its surface pretty firmly, the matter was evacuated through the opening of the bubo. Seeing, from this, that a communication existed between the two, I thought it the most eligible mode of proceeding, to pass a seton from the lower opening, and bring it out through the small boil. With this view, I injected a quantity of warm water through the opening of the bubo, and by this means distended the little tumour above, so as to guide me in making an opening into it with a common lancet; I then attempted to pass an eyed probe, armed with a skein of cotton, from above downwards, and from below upwards, but could not succeed in either way, owing to some membranous adhesions, which I found it impossible to break through with the probe. Foiled in this attempt, I contented myself with injecting a solution of the sulphate of iron, which passed readily from the one orifice to the other, and by the use of this, the sinus was completely and firmly healed in a few days.

Such are the principal points in the treatment of venereal chancres and buboes to which I think it necessary to advert. In an essay of this kind, much must of necessity remain to be learned from other sources; and of the more complete treatises on the venereal disease, there is none which my individual experience induces me to appreciate more highly, than that of the late Mr. Benjamin Bell, to which I have so often referred in the course of this little work. Upon the great question which has lately agitated the profession, respecting the employment of mercury, much information is yet to be expected from the army surgeons, and particularly from a work on military surgery, by my friend Dr. Hennen, now in the press. I have been favoured by the author with a manuscript copy of his chapter on syphilis, and can pro-



mise the profession, that it will be found replete with original and interesting observations.\* The doctor brings forward some very striking instances of the sufficiency and permanency of cures effected without mercury, while at the same time, he is far from denying the utility of this medicine in some cases of syphilis, as will be seen by the following passages, which I have the author's permission to copy:—"While I have enumerated many of the ill effects produced by mercury, when it acts as a poison, I must give my strongest testimony to the admirable results which proceed from its judicious use, in persons not constitutionally disposed to be injured by it, and who do not lead profligate lives, or are not exposed to the foul air of hospitals fully saturated with its fumes." "Of its unquestionable efficacy there can be no doubt, but its indiscriminate employment in every case, whether old or recent, suspicious or confirmed, and without any view to the patient's diet, or his general health, has produced the most dreadful consequences. To reduce its employment within the limits where it can be salutary only, without creating or evolving other diseases, is the best means of supporting the reputation of the medicine."

In adverting again to my own sentiments upon this subject, and to the urgency with which I have recommended mercury in the introductory part of this Essay, I trust I may be allowed to avail myself of the following observation of Dr. Percival of Dublin:—"The bulk of wise practitioners have all along adhered to the evidence of their experience, and have left the speculative part to contend for the palm of inventive ingenuity."†

\* Since this passage was written, Dr. Hennen's work has been published, and will, I trust, be found to merit the encomium I have bestowed upon it.

† Transactions of the King's and Queen's College of Physicians in Ireland.

ART. VIII. *Observations on the Efficacy of Emetics in the treatment of Hysteria, and on its Pathology.* By A. T. DEAN, M. D. of Chambersburg.

TO DR. EBERLE.

IN looking over the last number of the Medical Recorder, I was much gratified in meeting with your observations upon the utility of emetics in the treatment of hysteria.

This is a practice which I have pursued in that complaint for ten years back, and with a success far surpassing the ordinary mode of managing that disease.

Not recollecting to have seen emetics recommended in hysteria, either by the writers of this country, or those of Europe, I had considered the practice as new until I met with your remarks upon the subject. The observations of Dr. Smith in the Transactions of the New York Phycico-Medical Society, respecting their efficacy in this disease, I have not yet had the satisfaction of seeing. In this part of the country, the practice is undoubtedly a novel one, or at least as far as my observations extend, has not been adopted by any of our country practitioners.

Strongly impressed with the notion that the symptoms which characterise this disease are closely connected with a disordered condition of the stomach, I was early led to the adoption of emetics for their removal, and in the exhibition of these means, have seldom been disappointed in obtaining my object.

But your experience with emetics appears not to have extended beyond their administration in that form of the disease, which is distinguished by a torpor of the functions of animal life. In this variety of hysteria, I have repeatedly witnessed their decisive effects, and have no hesitation in declaring, that they will more speedily arouse the patient from a state of stupor and insensibility than any of the remedies which are commonly resorted to for this purpose. I have found them, however, no less efficacious in almost every other acute form of this eccentric disease. The more ordinary symptoms of globus hystericus, accompanied with weight, oppression, and fluttering about the breast, spasms of the bowels,



and the painful feeling of constriction, which hysterical patients not unfrequently complain of, as extending from the small of the back to the sternum, I have always found to yield immediately to the operation of a brisk emetic.

But it is in the chronic form of hysteria, in which the common routine of what are improperly termed antispasmodic medicines, produce no other than transient relief to the patient, that I have experienced the most permanent good effects from the administration of emetics. In cases of this description, where the patients had laboured under this disease for years, and during that time, had, by the advice and direction of respectable physicians, exhausted, with at most but temporary benefit, the whole class of remedies which are more usually prescribed, I have, by the continued exhibition of vomits, either entirely removed the complaint, or so far interrupted the habits of diseased action in the stomach, that antispasmodic and tonic medicines would, in general, complete the cure.

That the assemblage of morbid phenomena which constitute hysteria, have their origin either directly or indirectly, in a disordered state of the digestive organs, the symptoms of flatulence, acid eructations, hiccough, indigestion, constipation and wandering pains of the bowels, most unequivocally prove. These, together with the sensation of a ball arising from the left side of the abdomen, from thence ascending to the stomach, and finally to the pharynx, producing a sense of suffocation, are in a greater or less degree, the invariable precursors of an attack of this complaint.

But the stomach, though for the most part the primary seat of irritation in hysteria, is nevertheless, an organ through which diseased impressions made upon other parts of the system may be communicated to the brain. Thus hysteria, from the connection subsisting between it and certain irregularities of the catamenia, has been considered, from the earliest periods of our profession, as a uterine disease. In admitting, however, that functional derangement of the uterus may sometimes be the remote cause of hysteria, I am decidedly of opinion, that it is through the medium of the alimentary canal, that the brain and nervous system are made to partake of its irregular action.

In like manner, the passions of the mind, which are generally supposed to produce this disease by acting immediately upon the brain, I am inclined to think, exert their principal effects indirectly upon that organ by having previously thrown into spasmodic action the viscera of organic life, and especially the alimentary canal, which by its reaction upon the brain, nerves and muscles, gives rise to all the peculiar symptoms of hysteria.

The indescribable sensations which we instantly experience about the præcordia from violent emotions of the mind, the immediate suspension of our appetites, and the sudden interruption of the process of digestion upon the reception of afflictive intelligence, not only point out the powerful influence of the passions upon these parts, but show, in conjunction with the other attending phenomena, that, in the production of hysteria, their primary impression is chiefly upon the region of the stomach and bowels. So forcibly was Bichat struck with the control which the passions exercise over the organs of internal life, and particularly over the stomach, that he was induced to refer their origin to these viscera instead of the brain, the source from which they are more generally supposed to emanate.

In support of his opinion he has brought forward a number of interesting facts and arguments, which, if they do not establish his position, serve to prove the correctness of my observations in relation to this subject.

Thus, it appears, whatever may be the remote causes of this disease, and however deversified their source, they all invade the nervous system through the instrumentality of the alimentary canal, which, from its susceptibilities and extensive sympathies, is more liable to be influenced by the operation of morbid causes, than any other organ belonging to the body.

If then the stomach is so deeply concerned in the production and propagation of hysteria, would it not be more in accordance with a correct pathology of that disease to suppose, that emetics, by their direct and bold impression upon the stomach, at once revolutionize the condition, and through it, that of the various diseased organs which have been made to participate in its disordered functions, rather than to refer their beneficial operation to the



mechanical impulsion of the blood into the vessels of the head? But let us inquire into the correctness of your remarks upon the pathology of that variety of hysteria which is marked by interruption of the functions of external life. These phenomena, you ingeniously ascribe to a deficiency of cerebral action, or to a condition of the brain approaching to that which occurs in ordinary syncope.

That the morbid appearances, which are here presented, depend upon a paucity of blood in the vessels of the brain, and a consequent diminution of the action of that organ, will, I apprehend, admit of many doubts.

The violent convulsions with which the patient is, from time to time agitated, sometimes rendering it necessary to call in assistance to confine her to the bed, or to prevent her from sustaining injury from her own irregular movements, would in my estimation, warrant the conclusion that the brain is preternaturally excited. Even, when towards the close of the paroxysm, she lies in a soporose state, apparently destitute of motion, there is a general rigidity of the muscles, particularly of those about the jaws and abdomen, that does not at all comport with that relaxation of the system, which is usually brought on by a suspension of the functions of the brain from a debilitated circulation in that organ. The jaws are so firmly locked together, that it is with difficulty they can be forced open for the purpose of administering medicines. The sphincter ani is also on some occasions so closely contracted as not even to admit of the easy introduction of an injecting pipe into the rectum, when enemata are considered necessary for the relief of the patient.

Nor does the state of the pulse indicate such a condition of the brain. In syncope it is always greatly enfeebled, and sometimes altogether suspended, but in this form of hysterics, in which, you suppose the brain approximates so closely to its condition in fainting as to give rise to a torpor of animal life, the pulse, in point of strength and frequency, does not materially deviate from the healthy standard. On the contrary, it is sometimes so much excited, as to require the aid of the lancet to subdue it. This is more particularly the case in young robust females, who, from a sup-

pression of the menstrual discharge, are most subject to attacks of this disease. In some instances, it is indeed true, that the pulse, during the violence of the hysterical paroxysm, becomes small and irregular. Here the storm which pervades the nervous system, is, in some measure, maintained at the expense of vascular excitement, and is a proof of that law of the animal economy by which, when some parts are preternaturally stimulated, others are in consequence reduced below their natural action.

But if the suspension of relative life in hysteria depends upon a deficiency of blood in the vessels of the brain, how can we consistently account for the efficacy of cathartics in this, as well as every other form of that disease? To their good effects in hysteria, the experience of every practitioner must bear testimony. Chapman, Hamilton, and other writers of standing, place their chief reliance upon them. The latter gentleman depends exclusively upon purgatives in this complaint.

The insensibility and stupor of the patient, therefore, do not seem to arise from cerebral collapse. Nor am I inclined to believe, that they are owing to a compression of the brain from its being surcharged with blood. If that were the case, we should have the slow labouring pulse, the stertorous breathing, and the turgid countenance, which points out the existence of a fit of apoplexy. We should, in addition to these symptoms, expect to meet with a complete relaxation of the voluntary muscles, a condition the very reverse of that spasmodic rigidity with which they are affected in hysteria. What then, it will be asked, is that situation of the brain which gives rise to the foregoing symptoms in this disease?

An answer to this question, in the present uncertainty of medical theory, more especially as regards the pathology of that class of diseases termed *Neuroses*, must be involved in conjecture, and it would, perhaps, be more consistent with the principles of sound philosophy at once to acknowledge our ignorance, than to be advancing opinions, which must necessarily be speculative upon a subject so little understood. But as neither inanition, nor compression of the brain, will satisfactorily account for the symptoms which are exhibited in hysteria, these must depend upon some



other deviation from its natural action. This, I believe, consists in the functions of that organ being locked up by spasm.\* In this opinion, I am supported by the analogy of catalepsy, a disease nearly allied, in its causes and symptoms, to the complaint under consideration. In the history of a case of this most singular affection, which is given by Dr. Jebb of London, the patient complained of hysterical risings in her throat, flatulency, and an uneasy feeling of oppression about the stomach after eating, which, together with convulsive motions in the region of that organ, point out the close resemblance, if not the identity of the two diseases. In this patient, the Doctor informs us, that the attacks were now more frequent and violent about the time of menstruation, in which respect, the analogy between catalepsy and hysteria is still more apparent.

But whatever may be the precise nature of the change which takes place in the action of the brain, in this species of hysterical attack, certain it is, that symptoms of stupor and insensibility have existed in other cerebral affections, unconnected with any excess or deficiency of blood in the vessels of the encephalon. That this has been the case in epilepsy, hydrocephalus, and other cephalic complaints, can be shown from the most respectable authority. Abernethy gives the history of a case of hydrocephalus in which the brain, post mortem, presented no perceptible marks of disease, though there had been such a diminution of sensation as to remove all doubts with respect to the nature of the complaint. In this instance, there was great disorder of the stomach and bowels, the coats of which, upon dissection, were found to be much inflamed, but the brain, in every respect, was perfectly healthy in its aspect. Epilepsy has also existed without any morbid alteration in the structure, or appearance of the brain. The head of a person, who died of this affection, was examined by Mr. Cooper without his being able, from the most careful inspection, to discover any diseased appearances within the cranium. Abernethy

\* If the action of the brain, in health, consists in fibrous contractions, as the author of *Zoonomia* has ingeniously shown by his beautiful experiments upon ocular spectra, its fibres, like other contractile parts, will, of course, be susceptible of spasm from over stimulation.

informs us of similar examples which had fallen under his notice. He states further, that he had examined the brains of three persons, who died comatose in consequence of translated rheumatism, in none of which, were there any traces of disease to be observed, with the exception of some slight marks of inflammation in the pia mater.

Kirkland, moreover, has drawn the attention of physicians to a species of nervous apoplexy, in which, after death, there were no vestiges of disease to be perceived in the brain. The correctness of his remark upon this subject, has since been confirmed by the observations of other eminent practitioners.

These cases were all associated with a vitiated condition of the digestive organs. I have therefore, brought them forward to prove, that symptoms of carus may and do arise, in various instances, from an irritation existing in the stomach and bowels, that is productive of certain diseased actions of the brain with which we are, as yet, imperfectly acquainted, but which are neither dependent upon congestion, nor cerebral collapse: Not upon the former, because, in the preceding examples, dissections have shown to the contrary; and that they did not proceed from the latter, the other attending symptoms clearly evince.



## REVIEWS.

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Quidquid venerit obvium, loquamur  
Morosa sine cogitatione.

MARTIAL.

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**ART. IX.** *Traité de la Fièvre Jaune.* Par JEAN DEVEZE, Docteur en Médecine de la Faculté de Paris, Medecin du Château des Tuileries, &c. &c. 8vo. pp. 311. à Paris, 1820.

So much has been written upon Yellow Fever, that the subject has almost ceased to be interesting. Indeed, it is so intimately associated with the acrimonious controversies which have distracted the profession in this country, and which have so much disgraced the medical character in the estimation of the community, that it almost inevitably excites feelings of disgust. Yet there are circumstances connected with this publication, which are calculated to overcome our reluctance to engage in the subject of which it treats. It is a treatise upon the Yellow Fever of our country, from the pen of a foreigner. This must necessarily excite some curiosity in the minds of our readers. We therefore proceed to give them an analysis of its contents.

In a fulsome and egotistical dedication to the king, the author represents himself to his master as being very conversant with the disease of which he is to treat; and as having had extensive and frequent opportunities of observing its ravages in Philadelphia. Indeed, from his representation, the reader unacquainted with the truth of the case, would be led to believe, that yellow fever is as common in that city as it is in St. Domingo. In the preface, the author gives a brief account of his voyage to the United States,

&c. and informs us that he arrived in Philadelphia in August, 1793, just before the dreadful epidemic of that year. After some insolent and ill-natured remarks upon the prompt and judicious conduct of the college of physicians of Philadelphia on that dismaying occasion, he proceeds to tell us of the establishment of the hospital at Bush Hill; and of his appointment as physician to that charity, in consequence of the great noise produced by his cures. The remainder of the preface is occupied principally in setting forth his claims to the honour of being the first in this country who denied the contagiousness of yellow fever, and in complaining of the obstinate refusal of the American physicians to receive his lessons upon this subject. He concludes the preliminary remarks by an enthusiastic, but justly merited, eulogy upon the benevolence and intrepidity of Mr. Girard of Philadelphia.

In the introduction, he makes some reflections upon the dreadful nature of the disease in question; notices the opinion of some physicians, that it was known to Hippocrates; enumerates the different names by which it has been designated; objects to them all; and adopts that of *yellow fever*, as the one most generally received. He then proceeds to give an exposé of the plan he intends to follow in pursuing his subject. He divides the work into three books. In the *first* he gives the description of the fever as it appeared in Philadelphia in 1793;—exhibits its characteristics and its varieties;—details the usual appearances on dissection;—and gives some particular cases of the disease. In the *second*, he investigates its causes; defines infection and contagion; proposes some general laws respecting these two principles; applies these laws to the phenomena of yellow fever; and endeavours to determine its nature. The *third* book is occupied in detailing the treatment of the disease; and is divided into two chapters, the first relating to its cure, and the second to its prevention. He concludes the introduction by informing us, that the principal object of the book is to demonstrate the non-contagiousness of the disease; and that this is to be the principal point of consideration in the sequel. After this general view of the plan of the book, we are prepared to enter into an examination of its several parts.

The first chapter is introduced with a *topography of the city of*



*Philadelphia.* The author announces his intention to describe the city as it appears now, as well as its condition during his residence there in 1793. He takes much praise to himself for the many and great improvements which have been made in that beautiful city, of late years; and refers to a pamphlet published by him in 1794, as suggesting them almost all. (“Je me glorifie de les avoir presque toutes indiquées en 1794.”) His claims to the merit of having suggested all the improvements which have taken place in that city, do not very properly admit of investigation here. It cannot be improper, however, to state, that in a copy of this pamphlet\* we find it recommended: “first, that the interior of the city be cleared of *tan-yards* and *starch manufactories*; secondly, that the police particularly attend to the *cleanliness of the quays and streets*, to prevent the stagnation of the water in the ditches that are in the environs of the city, &c.; the same attention should be paid to the *markets*, to prevent *green and bad fruit* being sold, &c.” p. 136. He also advises that the *dead be interred beyond the limits of the city*, p. 138. And in p. 140, he suggests the propriety of supplying the city with water from the Delaware or Schuylkill. We will decline entering into an examination of his claims to priority in recommending these improvements: we presume his arrogant pretensions will be credited by none. But it will be seen in the sequel, that, so far as the first three measures are concerned, the author completely relinquishes the merit which he is here so bold in appropriating to himself. It may be well to observe too, that the fourth measure has never been adopted; and that Dr. Franklin† *first* directed the attention of the citizens of Philadelphia to the necessity of supplying the town with other water than that afforded by the pumps, and in his will directed the interest of a special legacy to be applied to that important object.

In his description of the site of the city, Dr. Deveze is, for the most part, very correct: we cannot say the same of every thing he

\* An inquiry into, and observations upon the causes and effects of the epidemic disease, which raged in Philadelphia, 1793. By Jean Deveze, &c. Printed by Parent, 1794. pp. 145.

† Dr. Franklin died in 1790.

says in this chapter. For instance, he is grossly incorrect in stating that the environs of the city of Philadelphia present nothing but a naked and uncovered waste; (*une terre nue et découverte*;) that the antipathy to trees is so great, that there is not a single public walk in the city, p. 14.\* We cannot but condemn the ignorance of the author on this subject, when we find him professing to give a topography of the city, with the view of deducing from it the most important and serious conclusions. His representations of the climate are equally untrue. "There is no country where the intemperance of the seasons is more constant than in Philadelphia." p. 14. "In winter, the air is sometimes so hot, as to make it necessary to withdraw from the fire and go out of doors to breathe; in summer, on the contrary, it is sometimes so cold, that one must be shut up and near the fire. The thermometer frequently varies 30 degrees in 24 hours, and even in one hour and a half." p. 14. We confess, that among all the complaints we have heard of the variableness of our climate, we have never heard any thing that comes up to this. Again, "the atmosphere is so charged with moisture, that one cannot be out of doors long at night without having one's clothes *completely wet*." ("*entièrement mouillés*." p. 15. No one can credit a statement so extravagant. In speaking of the manner of life of the inhabitants, he says, that their bread is hardly fermented, and very badly baked; that they are inordinately given to the use of ardent liquors; that they are very uncleanly, seldom changing their linen, keeping their beds in a very filthy condition, and seldom making use of the bath, p. 19. Now, it is a matter of universal notoriety that the bread of Philadelphia is proverbially very superior; and that a more temperate community, or a more cleanly city, cannot be found in the world. Among the many causes of insalubrity, he enumerates the numerous manufactories of *starch*, &c. which are situated in the heart of the city. But the author forgets, that this is one of the particulars in which he prides himself in having *improved* the city. The putrid miasmata which arise from the *filthy*

\* There are no less than *three* large squares reserved as public promenades.



*docks*, p. 12, and the *green fruit* eaten by the inhabitants, p. 18, he considers as additional sources of unhealthiness. We see then, that agreeably to the author's account of the present state of the city of Philadelphia, its condition can be no better than it was when he suggested the boasted measures for its improvement.

We know not whether to despise most the ridiculous vanity which led the author to claim the merit of having planned all the improvements of one of the finest cities in the world, or the want of candor he has evinced in so grossly misrepresenting the condition of the city he undertakes to describe.

The author proceeds to the *description of the fever*. After some general observations upon the atmospheric constitution and the prevailing diseases which preceded the epidemic, he describes it as consisting of three stages. There is nothing in his statement materially different from the histories which have been transmitted to us by Dr. Rush and others. Nor is there any thing particularly deserving of notice in the section relating to the prognosis and termination of the disease.

The second chapter treats of the characteristics of the fever, and of its changes. He sums up its peculiarities as consisting of a remittent type, a rapid and violent course, three distinct stages, and great inconstancy in its symptoms. He admits, however, that it is sometimes continued, and sometimes intermittent; that it may be more gentle and slow in its progress; that its stages sometimes succeed each other so rapidly, as not to be distinguished; and that, although a yellowness of the skin and black vomit are almost its essential symptoms, there are not wanting cases in which even these are absent. He concludes this chapter with a minute description of the yellow fever which ravaged Cadiz in the year 1800.

His third chapter, containing an account of his autopsic examinations, presents nothing new; and accords with the results obtained by other writers. He informs us that in some instances in which the violence of the disease had been very great, and its course very rapid, no traces of organic lesion, and indeed no marks whatever of disease, had been discovered by him on dissection.

The fourth and last chapter of this book, is taken up with the details of fifteen cases of yellow fever treated by the author at the Bush Hill hospital. With occasional slight variations, they are the same which the author published in the year 1794, in the pamphlet before noticed.

He commences the *second book* with a chapter on the *causes* of the disease. Plethora and a habit unaccustomed to the climate, he considers as its usual predisposing causes. Robust and sanguine constitutions he declares to be most liable to the disease. Women, he says, are much less exposed to its attack than men. So much so indeed that, according to Berthe's calculation of deaths at Cadiz, only 1577 females died out of 7387; and at Seville only 3672 out of 14685. Pregnancy he views as a predisposing cause of yellow fever. He says it is most apt to attack persons between the ages of 25 and 40 years; that old and young people are, for the most part, exempt from it; that certain trades have a predisposing influence upon the individuals engaged in them, whilst other occupations are wholly exempt. Fear too he enumerates as a powerful predisponent. The only necessary and exciting causes are, according to Dr. Deveze, *heat* and *infection*. These, he conceives, must be united in order to its production.

The next chapter is composed of two dissertations on *infection* and *contagion*. The object of the first dissertation is to show, that the effluvia of marshes, the putrid emanations from animal decomposition, and the miasmata which exhale from the bodies of a crowd of men confined in a small space, are precisely of the same nature, and differ only *in degree*; that intermitting fevers, bilious remittents, dysenteries, typhus, and plague, are only *grades* of the same disease; that they are always the product of some of these sources of infection; and that they are readily convertible from one into another. It would be no difficult matter to show that these, and many other minor positions advanced by Dr. Deveze, are wholly untenable. But we forbear to enter into their refutation, as he totally fails in his attempt to establish them.

He now enters upon the subject of *contagion*. "The word *contagion*," says he, "indicates the mode by which a sick person communicates to another individual the same disease with which



he himself is affected." We have no objection to make against this definition. It is correct and lucid, and admits of no misconstruction. But we cannot agree with the author in his description of the distinctive peculiarities of contagious diseases. "Contagious diseases are characterized by being communicated from individual to individual by means of immediate or indirect contact; and *by admitting of no other cause but this communication.*" p. 140. Now, independently of the *petitio principii* contained in this definition, it may justly be condemned as being at variance with established facts. It is acknowledged by all that *hydrophobia* is a contagious disease. But is it contended by any, that it is exclusively the product of animal contagion? Do not the best authors assure us, that it may arise spontaneously, that it may be induced by improper food, by long thirst, and by a variety of other causes in no way connected with contagion? Nay, do we not continually see cases of the disease occurring where it is impossible to admit the agency of contagion? Again, the contagious property of the *itch*, and several other cutaneous disorders, is known and acknowledged by all. But has the production of these diseases been restricted to the agency of their virus alone? Is it not known, on the contrary, that they may be generated by filth, by humoral impurities, and by many other causes? We might multiply objections of this kind against this clause of the definition. But those we have adduced are quite sufficient to expose its falsity. Having made some remarks upon the general laws which regulate the communication of contagious diseases, the author concludes the chapter by a brief survey of the peculiarities in the propagation of small-pox; and by informing us, that this disease is the *standard* he intends to adopt whereby to test the contagiousness of yellow fever. Grant the Doctor his positions, and the discussion is immediately set at rest. We know that small-pox is communicable under almost all circumstances; that it prevails in all climates, and at all seasons; "that frosts, and rains, and winds do not [*always*] interrupt its progress." p. 146. But no one ever contended for the contagiousness of yellow fever in the large sense in which we speak of the contagiousness of small-pox. And were we to admit small-pox as the standard of com-

parison, it would be impossible not to deny the contagious nature of syphilis, hydrophobia, and the itch. The question then is, not whether the propagation of yellow fever follows the same laws which regulate the communication of small-pox, or any other contagious disorder whatever; but *whether yellow fever is ever communicated from one individual to another by means of a virus peculiar to itself*. If so, it is contagious: if not, it has no such property.

The author proceeds in the third chapter to inquire, whether yellow fever is a disease resulting from infection, or whether it is contagious. "*La fièvre jaune est-elle une maladie par infection; ou bien est-elle une maladie contagieuse?*" For the purpose of rendering the discussion more clear, he divides the disease, considered as an epidemic, into *four* periods (*époques*): its commencement, its progress (propagation), its prevalence (*état*), and its subsidence (disparition). We follow the author through these several sections.

First, of its *commencement*. The author here takes a view of the circumstances which attend the beginning of an epidemic yellow fever—these he reduces to three: 1. The prevalence of a high *temperature*. 2. The existence of some source of *infection*. 3. The influence of *moral causes* over its extension. The first he acknowledges to be a peculiarity of yellow fever, in which it differs both from contagious and infectious diseases. This circumstance, then, cannot be objected to the doctrine of contagion; nor does it, by the confession of the author, go to establish that it is merely infectious.\* In considering the second circumstance attendant upon the origin of an epidemic, the author goes into an elaborate

\* We use the words *infectious* and *contagious* in different senses, in compliance with the usage in this country. There is no doubt, however, that these words are strictly synonymous in the writings of the best English authors. Dr. Johnson defines *infectious*, "contagious, influencing by communicating qualities," &c. And according to the same standard lexicographer, *contagious* means "infectious, caught by approach." There is a difference, however, between the meaning of *infection*, and the acceptance of *contagion*. The latter signifies the morbid *virus*; and the former is used to designate its action upon the body to which it is communicated. Accordingly the verb *to infect* means "to act upon by contagion," "to fill with



investigation to prove, what few have ever denied, that yellow fever does arise from infection, or in other words that it is produced by the action of a high heat upon putrescible materials of various kinds. The instances of its local origin adduced by Dr. Deveze, are quite conclusive on this point. But while they show that yellow fever *may* be produced in this way, they are far from establishing that this is its exclusive cause. We will not here enter into the discussion, whether yellow fever may arise from contagion:—our remarks on this point must be reserved for the section in which the author's chapter upon this subject will come under review. Suffice it, however, in this place, to state in reply to the doctrine of our author, that we have the authority of the best practical writers for contending that *solar heat alone* is quite adequate to the production of yellow fever. In considering the third circumstance observed in the commencement of an epidemic yellow fever, viz: the influence of *moral causes* in its production, the author instances the alarm which prevailed in Philadelphia in 1793, just before the memorable pestilence of that year, in consequence of the embarrassments of the bank; and the appearance of the epidemic of 1800 in Cadiz, soon after a disaster of the same nature. That moral causes do, in some degree, increase the susceptibility of individuals to an attack of the fever, there can be no doubt. But we think it rather fanciful to ascribe so much influence to the circumstances adduced in proof of the fact by Dr. Deveze; and especially as it is certain, that the epidemic prevailed most among the very class of the inhabitants who had no interest or concern in the affairs of the bank. But let us consider the author's design in noticing the influence of moral causes in exciting the disease. "This circumstance," says he, "is observed in all infectious disorders, but in not one contagious disease." Let us examine the truth of this assertion. The contagious character of hydrophobia is not disputed. But how does the assertion hold with regard to it? "Lively passions," says baron Boyer, "appear to favour the developement of this terrible something hurtfully contagious." The participle *infected*, applied to persons, means injured by the contagious poison;—applied to things, it signifies, imbued with contagion.

dy." (American edition of Boyer's Surgery, vol. i. p. 210.) And again, "it is likewise essential to inspire the patient with confidence, &c., concealing from him his situation, and promising a cure. Reason and experience alike establish the utility of this moral treatment," p. 216, vol. i. We might multiply authorities on this point, which show conclusively that contagious diseases are influenced by moral causes. But they must be familiar to every reader.

We are now prepared to estimate the author's conclusions, viz: That of the three circumstances which attend the invasion of yellow fever, two are distinctly referable to the laws of infection, and none to those of contagion. We have proved, we think, that not one of these phenomena is in any degree repugnant to the idea of its being a contagious disease.

He proceeds to consider the second period, viz: *its progress*. The circumstances which the author notes as characterising the propagation of the disease, are: 1. That its prevalence is at first restricted to the spot at which it first appears. 2. That its progress is gradual, (*pas à pas*) from the low to the more elevated parts of the town. 3. That its course is arrested by a wide street, or a public square. 4. That its extension is increased by means of large assemblies of people. Now, we confess, we can discover nothing in all this which does not perfectly accord with what might be expected from a contagious disease; nor can we conceive how the last circumstance can satisfactorily be explained but by the admission of this principle. As it regards the first circumstance, it may be remarked, that if it is admitted as an argument against the contagion, it would be equally valid against the infectious nature of yellow fever. Supposing for a moment the disease to be infectious only, in what way should the phenomenon be explained? Certainly it admits of no other explanation than that given by the author:—that the infectious miasmata have not yet become extensively diffused. And upon the supposition of its contagious property, is it not equally philosophical to account for the phenomenon by the yet limited diffusion of the contagious effluvia? The second circumstance will admit of the same explanation, and certainly militates as much against one theory as



the other, if admitted as an argument against either. But, says our author, a contagious disease, such as small-pox, jumps from spot to spot; and every individual sick of the disorder becomes a centre of contagion. But, adds he, this is not the case in yellow fever. The author certainly will not contend that cases of the fever may not occur in various and remote parts of a town during its prevalence in a particular spot. A case of this nature he himself records in the very page under review. How then can he say that the disease never leaps from spot to spot? And as it regards the second particular of the assertion, it is very certain that the case he adduces to prove it, is very far from being satisfactory. His deduction from it is a perfect example of a *non sequitur*. The third circumstance the author explains by the frequent renewal of the air in a spacious street, or a public square, whereby the infectious particles are prevented from accumulating in sufficient quantity to become noxious. *Mutatis mutandis*, this is precisely the explanation we would give, believing as we do the disease to be contagious. And we see no good reason why it should not be admitted. The author is aware that the fourth circumstance is in favour of the doctrine of contagion. "Je conviens," says he, "que la dernière circonstance que j'ai signalée de la propagation de la fièvre jaune est *tout en faveur de la contagion*," p. 188. But he contends that it may be equally well explained upon the principles of infection—that as each individual in the assembly is a centre of infection, there is no difficulty in accounting for the increased extension of the disease. Now, as the author in the very preceding page contends that the morbid principle of infectious diseases cannot be transported from place to place by men or things, p. 187. we are left to suppose every individual in the assembly as already diseased—which reduces the whole reasoning to an absurdity. But let us ask Dr. Deveze, whether, in his view, the health of a large body of people would be endangered, by having in the assembly any number of persons sick of *intermittent fever*? We really believe he would laugh at the fears of those who apprehended any danger from this source. After this examination of the laws which seem to regulate the progress of an epidemic yellow fever,

we cannot admit the author's conclusions, that in one circumstance alone it has affinity to contagious diseases, and that in all four it perfectly accords with the principles of infection. We think we have shown, that not one of the four militates, in any degree, against the probability of its being contagious.

We come now with the author to the third period of the epidemic, viz: its *prevalence*. We cannot find that any thing advanced under this head tends to show that yellow fever is not contagious. That winds, and rains, and variations of temperature, modify its epidemic character, and even have an influence upon individual cases, is readily admitted. But the author certainly attributes too much to these circumstances, and is too exclusive in restricting their influence to epidemics of an infectious character. "It has been observed," says a standard authority, "that while contagious distempers rage, certain states of the air are more or less favourable to their progress." Wilson on Fevers, vol. i. p. 157. "It is generally supposed, that hot weather is favourable, and cold weather unfavourable, to the spreading of contagious diseases; and this, with some exceptions, is true." Id. p. 158. So well convinced was Dr. Sydenham of the influence of the atmosphere and the weather over contagious diseases, that he says, that *measles* require a peculiar constitution of air to propagate itself, and that it is epidemic only in the vernal months, and ceases in July.\* That other contagions are controlled by the weather, we will further see in the next section. The modification of the epidemic yellow fever by meteorological circumstances, is, therefore, no argument against the contagious character of the disease.

We now come to the fourth and last period, viz. its *subsidence*. The phenomena noticed at this period, are—1. That the disease gradually retires in a retrograde course. 2. That it is checked by the appearance of frost. The first circumstance needs no particular consideration. We have shown above, that its gradual extension is perfectly in accordance with what might à priori be expected of a contagious disease. Its gradual subsidence is

\* Works, Sect. 4. Chap. V.



readily accounted for upon the same principles. As it regards the second circumstance, we must observe that the phenomenon has been remarked in other diseases, the contagious nature of which the most sceptical have never doubted. Accordingly the distinguished author above quoted, remarks:—"it is common for contagious diseases to suffer a check, or cease altogether, when the winter sets in." (Wilson on Fevers, vol. i. p. 158.) Dr. Willan says, that scarlatina anginosa, and small-pox are checked by the first frost in December. (Diseases of London.) And the same fact, if we are not greatly deceived, is noticed by Dr. Sydenham.

We have thus minutely examined all the laws which the author has laid down as regulating the rise, progress, duration, and subsidence of epidemic yellow fever; and, we think, we have clearly shown that not one of these laws is exclusively peculiar to infectious diseases; that some of them belong to diseases of a contagious nature only; and that, therefore, *yellow fever is a contagious disease.*

We shall pass over the remainder of this section. Its object is to prove that yellow fever is not a disease *sui generis*, but only the *maximum* grade of diseases arising from infection. We do not believe this doctrine, but will not detain the reader by examining it, as we conceive it does not materially affect the question of the contagiousness of yellow fever. We, therefore, proceed to the consideration of the next section, entitled—*Proofs of the non-contagiousness of yellow fever.*

It would extend our remarks to an unbecoming length, were we to examine all the arguments advanced by Dr. Deveze on this subject. His observations are very prolix and very minute; but, in our opinion, they are very far from proving the doctrine he maintains. Suffice it, then, to give a very brief summary of his arguments, and concisely to state the reasons why we believe that yellow fever is contagious. He advances, that, although St. Lucia, Martinique, and St. Domingo, are the most unhealthy of all the West India islands, there is in the first an islet in which the fever never appears, although a constant communication is maintained between it and the island; and that in the last two, there

are districts equally exempt from its ravages. He states farther, that during the epidemic of 1793, many of the inhabitants of Philadelphia fled from the city, and that although many of these were already infected, in no instance did they communicate the disease to their attendants in the country; that in the Bush Hill establishment none of the patients labouring under other disorders, became affected with yellow fever; that at the end of the epidemic, the board of Health of Philadelphia sold to the French government all the beds, furniture, and utensils belonging to the Bush Hill hospital, for the use of their soldiers, but that none of these ever took the disease. He lays great stress upon the unsuccessful attempts of Dr. Ffirth to communicate the disease by inoculation, by inhaling the vapour of black vomit, and by swallowing the inspissated residuum after its evaporation. This is the amount of his arguments against the contagion of yellow fever. Its insufficiency must be apparent to the most prejudiced abettor of infection; and every reader conversant with the subject, must know that these arguments have all been repeatedly combated, and we do not hesitate to say, *refuted*.

Before presenting a statement of the reasons which induce us to believe the yellow fever contagious, we must impress it upon the reader, that we view the doctrine of the *domestic origin* of yellow fever as totally unconnected with the point in controversy. We are not prepared to deny that the disease can originate here, although we must say, that there are very strong reasons for believing that it does not; yet we believe firmly, that when it arises among us, whether it be the product of domestic filth, or whether it be brought from abroad, it is decidedly *contagious*. Let us give our reasons for the belief.

I. Because its importation on various occasions can be distinctly proved. II. Because it can be proved that the disease has been transmitted from individual to individual, under circumstances which forbid the supposition of the agency of any cause but a specific contagious principle.

I. From the innumerable occasions on which the *importation* of yellow fever into the United States has been shown, we shall select the most familiar, and the most conclusive. We, therefore,



invite the reader's attention to the history of its introduction into New-Haven in 1794; into Providence in 1805; into New York in 1819; and lastly, into Middletown in 1820.

On the 10th of June 1794, the yellow fever appeared in *New-Haven*. Several deaths having occurred from this disease, the select-men of the town were requested by the inhabitants to institute a diligent inquiry into its origin. The investigation resulted in the conviction of every body, that the disease had been brought from Martinique by the sloop *Iris*, captain Truman. The facts which led to this opinion, were, that no person had been sick of yellow fever before the arrival of this vessel; that this vessel lay at the wharf at which the first cases had occurred; that her captain and crew had all been sick of the disease in the West Indies; that she had brought home in a chest the clothes of one of the seamen who had died of yellow fever in Martinique; that these clothes, among which were some which this man had used during his illness, were unpacked in the presence of four persons, three of whom sickened and died very shortly after this exposure; and finally, that no individual sickened in the whole city, unless in consequence of attending the sick, or of exposure to some source of contagion. These plain and conclusive facts remained undisputed until nearly five years after this period, when Mr. Noah Webster attempted to call in question the correctness of some part of the statement. For this purpose he produced the testimony of the captain and the mate of the *Iris*, to prove that the clothes worn during his illness by the mariner who died in Martinique, were buried with him, and not returned on board the vessel; and that all the clothes in his chest were new. The statements contained in the depositions of these persons, have, however, been unequivocally denied by others, and been proved to be *false* by the affidavit of colonel William Lyon of New-Haven. This gentleman had been appointed upon a committee of the common council, to oversee the removal and destruction of the said chest; and he declares positively that an *old dirty* blanket, a pair of *dirty* trowsers, and *other old clothes*, were contained in it. The affidavit of Mr. Ives, a gentleman of uncorrupt integrity, states too that the mate of the *Iris* had assured him, that the *blanket*

which had been used by the mariner who died in Martinique, and the clothes in which he died, *were locked up in the said seaman's chest* by himself, (the mate.) These are all the objections that have ever been advanced against the introduction of yellow fever into New-Haven in 1794. We think, we have given enough of the history of the disease on that occasion, to show distinctly that it was *imported*. But for more ample and satisfactory details, we must refer the candid reader to the "additional facts and observations relative to the nature and origin of the pestilential fever, by the college of physicians of Philadelphia." p. 50. 1806.

The evidence of the importation of yellow fever into *Providence*, (R. I.) in 1805, is equally striking and irresistible. It would be impossible to give a minute history of that epidemic. Let it suffice to give a summary of the principal facts upon which its importation rests, and to refer for particulars to the statement of the venerable and distinguished Dr. Bowen of Providence. Let it be remarked, then, that at the time the fever made its appearance in that city, and for a long time before, the town was remarkably healthy; that the district in which the disease appeared was remarkably clean and free from filth; that, with three or four exceptions, all the cases occurred in the neighbourhood of the wharf at which foreign vessels usually unloaded; that vessels were permitted to come up to the town without cleansing, or performing quarantine; that immediately before the commencement of the alarm, three vessels, on board of which yellow fever had prevailed either in the West Indies or on the homeward passage, had arrived in the port, and deposited their cargoes at the wharves in the immediate vicinity of the first cases which occurred; that, upon the removal of these vessels and the inhabitants of the sickly district, the disease disappeared entirely, except in two cases; that one of these persons had had communication with one of the vessels after her removal, and the other had visited in the street in which the disease was prevailing; that upon the return of the inhabitants to their homes in two or three weeks, and this too in the middle of August, not one of them became sick, although no measures had been taken to alter the condition of the district. Such are the strong facts afforded by this epidemic in



proof of the importation of yellow fever. If they fail to convince, no amount of evidence, however clear, can be satisfactory.

We come now to the epidemic which made its appearance in *New-York* in 1819.

The facts upon which rests the evidence of the importation of this epidemic, are, that the wharf near which the disease appeared, was occupied almost exclusively by Baltimore coasters; that at the time of its appearance here, the yellow fever was prevailing to an alarming extent in Baltimore; that on the 2d of August, the sloop *Hiram* arrived at this port from Baltimore with three persons sick on board, one of whom died at the quarantine of decided yellow fever; that the said vessel had lost one man on the passage, who was considered as having this disease; that after a detention of only four days, and without undergoing any purification at the quarantine, she was permitted to come up to the city and land her passengers and cargo; that one of her passengers, Mary Stevens, took lodgings at Mrs. Kavenaugh's, who shortly sickened and died of yellow fever, as there is reason to believe, from washing the clothes of the crew, and of the person who died at sea; that several other persons, who were the earliest victims of the epidemic, had frequent communications with the house of the said Mrs. Kavenaugh; and that, with two exceptions, every case of the disease which occurred that season, was distinctly traceable to imprudent or necessary visits to the sickly district. Attempts have been made to destroy the conclusive character of these facts, and to represent some of the most material of them as false. Let us examine the objections which have been made to the above statement. It is said that a respectable person gave assurances that Mrs. Kavenaugh never took in washing; that she had no yard, and consequently no convenience for the purpose; that no person of the name of Mary Stevens lodged with her at this time. It is also stated, that the captain testifies on oath, that, at the time he left Baltimore, no yellow fever prevailed there; that the person who died on the passage, died in convulsions, in consequence, as he supposed, of intemperance; and that the clothes he wore during his illness, were thrown overboard. It is stated, further, that the person who died at the quarantine, died of bilious remittent

fever; and that the other two who arrived sick both recovered, the one having nothing more than a mild remittent, and the other an intermittent. We would beg the candid reader to consider for a moment, whether the information received from an unknown person, whose name even is not given, is of any weight in comparison with the positive assurances of the mate of the Hiram.\* We would further, beg him to consider, whether Mrs. Kavanaugh's having no yard to her house, is any evidence that she had no conveniences for the purpose of washing. As it regards the testimony of the captain, it completely destroys itself. He states that he left Baltimore on the 2d of July, and that no yellow fever was prevailing when he sailed from that port. Now, we appeal to the common sense of the reader to determine, whether it is credible that the Hiram had a passage of *thirty-two days* from Baltimore to New York!† or whether it is in any measure probable, that the clothes of a man who was believed to have died of convulsions from *intemperance*, would have been thrown overboard! On both these points the captain and the mate are at issue,—as the latter declares that the man who died at sea was ill, as far as he could judge, of the same disease which prevailed at Baltimore when the Hiram left that port; and that his clothes, so far from being thrown overboard, were brought to the city and washed by the persons before referred to. As it respects the objection, that the young man who died at quarantine had nothing but bilious remittent fever, we have only to say, (and we say it upon the authority of the present health officer of the port,) that his case is recorded upon the books of the quarantine establishment as a case of “*malignant fever*.”

We have thus, we think, entirely refuted all the objections advanced against the probability of the yellow fever's having been imported by the Hiram; and made it clearly to appear, that the

\* All the non-official information respecting the Hiram, was furnished by the mate to Dr. Hicks, an agent of the board of health of New-York. See Dr. Hicks's letter to the board, dated November 2, 1819.

† The fact is that she left Baltimore *late in July*; and, before sailing, lay at Smith's dock, where the disease was prevailing.



epidemic which prevailed in New-York in the year 1819, is fairly ascribable to *imported contagion*.

But the history of the yellow fever which appeared in *Middletown*, (*Con.*) in 1820, is, if possible, more conclusive on the subject of importation, than any which we have yet considered. Here no objections have been raised against the official history with which we have been furnished; no quibbles, resorted to, to evade the force of facts. Every unprejudiced man is convinced of the introduction of the fever from abroad on this occasion; and those who, blinded by the zeal of party, persist, in defiance of the clearest evidence, in denying its importation, evince the irresistible force of this testimony by submitting in humble *silence* to their inglorious defeat. We present our readers the following extract relative to the Middletown fever, from Dr. Beck's report\* to the board of health of New-York, confident that the conviction of its importation in this instance, at least, must fasten upon the minds of the intelligent and the candid. After a lucid detail of the cases which had occurred, in which it appears they were almost all traceable to a diseased vessel from the West Indies, Dr. Beck sums up the evidence of the introduction of the fever from abroad, in the following words:—"Such is the detail of the circumstances connected with the several cases of fever that were reported. In looking around the city for any domestic cause which might have produced them, I found none that was adequate. Middletown is distinguished for the salubrity of its situation and the health of its inhabitants. It is built on the slope of a beautiful hill, and most of the houses are at considerable distances from each other. The freest circulation of air pervades every part of it. The town is cleanly, and even along the river and wharves, there are no accumulations of filth of any kind. The ship-yard in the neighbourhood of Mrs. Child's house (about which something has been said to induce the belief that it was very filthy,) was uninclosed, and had nothing in it but a small quantity of timber scattered over its surface. There was nothing about this place which even the most distempered imagination could convert into a source of infec-

\* Dated July 17th, 1820.

tion. The same will apply also to a distillery of rum in the neighbourhood.

“Nothing seems capable of explaining the facts on this subject, but to admit that, with the exception of the first two cases, which occurred anteriorly, the existence of malignant fever in Middletown, was owing to the presence of this vessel from the West Indies. Otherwise it is unaccountable why, in a place where there is a total absence of all the causes contended for by those who maintain the doctrine of domestic origin, and at a season too, which is there usually the healthiest in the whole year, so many cases of a malignant fever should occur so rapidly in succession. Besides, upon any other principle it must seem marvellous that the march of disease should proceed with the progress of this vessel up the river. She lay opposite to Middletown, and the fever followed—she lay opposite the factory, and the fever made its appearance—she proceeded to Hartford, and there we have the official report of the physicians to the board of health, that at least one case of malignant fever occurred which was traceable to this vessel. It is true that all the cases cannot be distinctly traced. This, however, is immaterial. The fact that somehow or other the disease was connected with the presence of this vessel stares us too broadly in the face to admit of a denial.”

In this extract, we think there is sufficient to convince any candid mind of the *importation* of the fever into Middletown in 1820. For more ample satisfaction on this subject, we refer the reader to that able document.

We have thus proved the *importation* of yellow fever on various occasions. Upon its *communication* from individual to individual, we might dilate at very great length; but the length to which we have already extended our remarks, requires that we should be very brief. We shall therefore confine ourselves to the cases of contagion at *Germantown* in 1798, as related by Dr. Wistar, and to those recorded as occurring at *Staten Island*, by Bishop Moore, of Virginia.

On the subject of the cases at *Germantown*, we could not do better than transcribe the simple narrative to be found in the “Additional Facts and Observations, &c. by the College of Phy-



sicians of Philadelphia," published in 1806. "The disease which produced the fatal effects now to be related commenced in the family of Elizabeth Johnson, a widow, who lived in the main street of the village of Germantown, about six and a half miles from Philadelphia. The first person attacked was her child, Betsey Johnson, who had been in Philadelphia from the 3d to the 7th of August, in a neighbourhood where several cases of the fever had already appeared. She returned home the 7th, and on the 9th of the same month was attacked with the yellow fever, which terminated fatally in four days. Fourteen days after, viz. August 27th, Mrs. Duy, the next neighbour of Mrs. Johnson, who had visited Betsey several times during her illness, was attacked with a fever supposed to be of the same kind, and died at the end of four days. On the 30th of August, the wife of Charles Hubbs, who also lived near to Mrs. Johnson, and had visited both Betsey and Mrs. Duy, once at least during their illness, *but had not been in Philadelphia for many months*, was attacked with unequivocal symptoms of yellow fever, in its most malignant form, and died on the 2nd of September. Mr. Duy, husband of the above mentioned Mrs. Duy, was attacked eighteen days after the death of his wife, and died also, after an illness of six days. A few days after the death of Mrs. Duy, an English gentleman and his wife, of the name of Fisher, who had fled from Philadelphia on account of the fever, went to board with Mr. Duy, and were placed in the chamber occupied by his late wife; they were also attacked with the fever. Mrs. F. recovered in a few days, but Mr. F. died with black vomit. At the same time the disease re-appeared in Mrs. Johnson's family, in a young female servant, who was very ill, but recovered. Soon after the attack of this girl, Mrs. Johnson herself was taken ill of the same disease: she had visited both of her neighbours, Mrs. Duy and Mrs. Hubbs, while they were sick; she had also assorted the clothes of her deceased daughter, four or five days before her own attack commenced, but had not been in Philadelphia for a month. Her disorder continued eight days, and terminated the 28th of September with convulsions and the black vomit. A few days before the death of Mrs. Johnson, Elizabeth Stern, a woman who lived in the family, was attacked

with the fever and died. The wife of a tenant of Mrs. Johnson, who lived in a separate part of the house, but used the same yard, was attacked before the death of Elizabeth Stern, and recovered with great difficulty. The last victim to be mentioned, was one Stephen Post, an old man, who lived at a distance, but worked in Mr. Duy's barn while the bed was there on which Mrs. D. died. He died in a few days. These melancholy circumstances occurred in a village which has long been remarkable for its salubrity, at a time when the other inhabitants enjoyed their usual health. In most of the cases, the disease appears to have been contracted at the house of Mrs. Johnson, which, before this distressing period, had been eminently distinguished by the health and longevity of its inhabitants. The family were extremely neat, and it may be asserted with confidence, that the premises were never more clean than they were at the time of this truly affecting catastrophe." We may well ask, with the author of this document, "what cause but *contagion* is adequate to the production of such a disease among persons so situated?"

Nor will the facts recorded by Bishop Moore be found less decisive on this point. In the statement given to the world by that venerable prelate, relative to the contagious nature of yellow fever,\* it will be seen, that in the year 1799, a person who had been taken to Staten Island sick of the disease, communicated it to his physician; and that this physician subsequently communicated it to his wife. A stronger proof of its communicability cannot be demanded. The Bishop tells us in the same paper, that he himself contracted the disease in 1803, from a young man who had been seized with yellow fever in the city, and had been transported to Staten Island. It is worthy of remark, that he had not been in New-York in many weeks before his illness, and that the young man from whom he received the disease, was confined in an airy well-ventilated chamber. In addition to the above, Bishop Moore states three other cases in which yellow fever was evidently conveyed from individual to individual, under circumstances which forbid the supposition of the agency of any

\* See the American Med. and Philosophical Register. vol. II.



cause but a specific contagion. We might multiply examples, but these must suffice.

We have thus endeavoured to prove, that on various occasions the yellow fever has been *imported*; and that it may be transmitted from person to person by *contagion*. In the brief summary we have given of the arguments on these points, we have selected the most striking and undeniable facts. Innumerable facts of the same kind, and on equally unquestionable authority might be produced, but the limits to which we are necessarily confined, forbid further details; and one well established instance of its communication is sufficient to prove its contagious character.

The rest of Dr. Deveze's work is devoted to the *treatment* of the disease. There is nothing very novel or striking in his practice, although he boasts so much of his having obtained the preference over the American physicians during his residence in Philadelphia. In the first stage he recommends diluent drinks, such as lemonade, veal broth, barley water, &c. He advises moderate and cautious blood-letting, warm bathing, enemata, applications of cold water to the abdomen, and gentle anodynes. In the second stage, he advises the use of camphor and nitre, decoctions of serpentaria and peruvian bark, blisters and sinapisms, gentle laxatives, and sometimes emetics. He thinks too, that wine may be allowed here. In the third period of the disease, he recommends a repetition of the medicines given in the second stage, diascordium, nitre, warm bricks to the extremities, flannels wet with camphorated spirits, &c. We might, perhaps, find fault with some points of Dr. Deveze's practice, but his work has already occupied so much space, that we cannot properly extend our remarks upon it to a much greater length. This reason too must forbid our entering into an examination of the prophylactic measures which he recommends, and of the report of the committee of the Institute upon his Memoir on the Contagion of Yellow Fever.

We here close our remarks upon Dr. Deveze's book, by expressing our opinion, that it is decidedly one of the most able and ingenious works which have appeared on the side of the question which he espouses. We have to regret, however, that Dr.

Deveze has not always shown that courtesy to the physicians of this country, which the hospitality extended to him while a destitute and forlorn stranger among us, should have secured; and that he has not evinced that candour which, disdaining the narrow spirit of national prejudice, is ready to acknowledge merit wherever it may be found. It cannot be pleaded in extenuation of his injustice, that he was unacquainted with their writings:—the writings of Rush, Miller, Hosack, Bayley, Webster, Lining, Addoms, Caldwell, Currie, and Cathrall, are known to all; and he shows too much familiarity with their doctrines and their mode of handling the subject, to have any claim to the apology of ignorance. D.

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ART. X. *Lectures on Physiology, Zoology, and the Natural History of Man.* Delivered at the Royal College of Surgeons, by N. LAWRENCE, F. R. S. Professor of Anatomy and Surgery to the College; Assistant Surgeon to St. Bartholomew's Hospital; Surgeon to Bridewell and Bethlem Hospitals, and to the London Infirmary for Diseases of the Eye. London. Printed for J. Callow, Medical Bookseller. 1819. pp. 579.

THE book, the title of which is placed at the head of this article, excited, at the time of its publication, a degree of interest in the literary and scientific circles of Great Britain which has been rarely the lot of the productions of medical men. This arose from the interesting nature of the topics which are discussed, the manner in which some of them are treated, and the splendid talents and high professional rank of its author. We can hardly conceive of stronger evidence of the high endowments of this gentleman, than the fact of his having been placed in so many important and responsible public stations, many of which too, we understand, are under the immediate patronage of the government and the church, notwithstanding his sentiments in politics, and religion. But Mr. Lawrence is not only opposed to the prevailing opinions both in church and state, but, if we can form a just idea of his character from his book, he is not a man who



would take much pains to conceal sentiments. At any rate, this remark is very strikingly true in his book.

Our readers probably know, that the magnificent anatomical museum of Mr. John Hunter, was purchased at his death by the British government, and placed under the care of the Royal College of Surgeons of London. Suitable rooms have been prepared for it, and public lectures, which are illustrated by these preparations, are now given annually, to which none are admitted but practitioners, and those students who have spent at least one winter in anatomical pursuits. The lectures, to which we are now about to call the attention of our readers, were delivered by Mr. Lawrence as a professor in this college, and we are informed, that he deemed their publication necessary, in order to vindicate him from "the charge of having perverted his honourable office to the very unworthy designs of propagating opinions detrimental to society," which was brought forward by Mr. Abernethy. A large edition was accordingly thrown off. It would seem, however, that Mr. Lawrence's friends did not agree with him in believing that it was very well adapted to attain this end, for soon after it was published, and after about 500 copies had been disposed of, in various ways, he was prevailed upon to suppress the remainder of the edition. Nor can we much wonder at the prudent counsel of his friends, when we find his book abounding in opinions directly hostile to the religious and political tenets of the predominant party in his own country.

Mr. Lawrence complains, that very unfair and unwarrantable measures had been had recourse to for exciting popular odium against him;—that his opinions had been misrepresented; that he had been stigmatised by injurious epithets, and that the doctrines which he taught were proscribed as subversive of religion and government. He defends himself on the ground of the necessity of freedom of inquiry in scientific pursuits.

We hold in the highest veneration and respect every thing which is connected with revealed religion. We believe that the religion of Jesus Christ is entitled to the most full and unqualified belief; that it is emphatically divine. Nevertheless, we cannot help thinking that this noble institution has been injured by the ill-

timed officiousness of some, who have considered themselves its exclusive friends;—by those who, from a blind attachment to their own peculiar opinions, and an intolerant contempt of those of others, denounce all who differ from them in the slightest degree, as hostile to religion itself; by men who, too ignorant and narrow-minded to understand its full scope and aim, look to the Bible, not merely as containing all those rules and principles by which we should be governed in relation to our neighbour and our God, but likewise, as the only true test to which theoretical opinions in natural science should be submitted:—men who, if they had the same power, would abuse it as the Caliph Omar did; who, when he ordered the Alexandrian Library to be burnt observed, “if all its useful knowledge is contained in the Alcoran it is unnecessary, and if it differs from it, it is impious and deserves to be destroyed.” Such persons are seldom disposed to enter upon a fair and dispassionate defence of their opinions. Knowing the strong disposition there is among men to be carried away by names, they substitute invectives for reasoning, and epithets for arguments. But religion is much too strong in herself to need such defence, or such defenders; she does not shrink from the most scrutinising examination, but invites inquiry; she has nothing to gain from dogmatism and concealment, but every thing to hope from a liberal spirit of investigation. There is nothing about her to sanction this exclusive, proscribing spirit. Her business is with the heart, her object is to kindle within the breast all the generous, kind, and benevolent affections, and to elevate and dignify the human character. The object of Christianity can in no degree be understood, from any thing which appears about it, to instruct us in physical science, or profane learning. It nowhere pretends to the exactness of scientific modes of expression, nor can we understand how any fair-minded man should consider obscure allusions, or expressions, purely incidental, and suited no doubt to the previous impressions and comprehensions of the persons to whom they were addressed, as sufficient to settle questions in science. If this be the rule by which our investigations in physical science are to be graduated, then there is an end to inquiry. We must no longer seek to become acquainted with the properties



and functions of organised bodies in the dissecting room, but we must endeavour to do this through the medium of theological distinctions, and biblical criticism.

But if we would rank ourselves among those who will contend to the last for freedom of inquiry, and a right to draw those inferences which naturally and fairly flow from well ascertained facts, in matters of science, without stopping to inquire whether they favour or oppose any given theological dogmas; because we are impressed with the belief that this is as much the interest of religion as it is of science; yet we cannot express ourselves in terms of reprobation too strong, against those whose leading, or even subordinate object in philosophical investigation, is to impugn religion: who, while their whole aim seems to be to elevate the character, and extend the limits of human knowledge, and to enable men to form more just opinions concerning their own physical and moral nature, and the objects which surround them, are endeavouring by direct or indirect means to sap the foundations of that religion which we conceive to be by far the most valuable of our intellectual possessions. Now we conceive that the friends of Christianity have just ground of complaint against Mr. Lawrence, for his conduct in this respect. Not that we think there is any thing so very reprehensible in his opinions on thought, or the causes of the varieties of the human species, so long as he confines himself to these speculations, which may, or may not be true, and which have been in turn defended and opposed by men equally distinguished for their learning and piety. But what we chiefly complain of is, his sneers at religion, and the political allusions which we so frequently encounter, and which are, so far as we can understand, entirely superfluous and foreign to the objects of his lectures. But although we disapprove of these parts without any qualification, and cannot help expressing our surprise that a person, of his fine understanding and good taste, should have allowed so beautiful a work to be marred by such useless and offensive excrescences, yet we have derived too much instruction and pleasure from these lectures not to judge of their author with charity. We can only say that Mr. Lawrence seems to us to be a living example of the tendency of the human mind to go to extremes, and a striking in-

stance, that persons possessing the finest intellect are not always exempted from those infirmities, which usually characterise the most vulgar;—that, in a word, fanaticism is not confined to the ignorant or the religious, but that a man may be a bigot even in philosophy.

We contend that truth is the only legitimate object of scientific research; and he who would be a successful votary of truth must devote himself to her cause with an entire singleness of mind. He must pursue the clue without hope or fear of the point to which it may lead. He must endeavour for the time to lay aside every prepossession.

When this is done with intelligence and candour, we are authorised by experience to say, that the result will be as beneficial to the cause of morality and religion, as it is to science.

The first lecture is a reply to the charges of Mr. Abernethy. The second is on the study of Zoology and Comparative Anatomy.

The first topic discussed by Mr. Lawrence, to which we shall call the attention of our readers, is life. After stating the various explanations which have been advanced concerning life, and showing the insufficiency of each, he abandons the subject without attempting to give any hypothesis of his own. The following, however, we conceive to be a very lucid statement of the phenomena which are found connected with life.

“The body is composed of solids and fluids; the former arranged in fibres and laminæ, so as to intercept spaces, which are occupied by the latter. The solids give the form to the body, and are contractile. The fluids are generally in motion.

“Such a kind of composition, and such an arrangement of the constituent parts, is called organization; and, as the vital phenomena are only such motions as are consistent with these material arrangements, life, so far as our experience goes, (and we have no other guide in these matters,) is necessarily connected with organization. Life pre-supposes organization, as the movements of a watch pre-suppose the wheels, levers, and other mechanisals of the instruments.”

That restless, inquisitive something, called mind, which is never quiet, but constantly in action, which endeavours to compre-



hend every thing, from the most minute particle of matter that floats upon the air, even to Deity itself, could not fail to be attracted by its own miraculous existence and properties. This subject, which offers so ample a field for speculation, is taken up by Mr. Lawrence and briefly examined, under the head "Functions of the Brain." We shall state in his own language his opinions on this point. We think it proper to do this, because it is necessary to understand his opinions and reasonings on this subject, in order fully to comprehend his discussion of many of the subjects taken up in the remaining parts of the lectures.

"If we contemplate living beings arranged in one line, beginning with the most perfect, and continue downwards, we find a tolerably regular gradation from complicated to simple, through the whole series. At the one end is man, at the other an animated microscopic point, of which thousands are found in a single drop of fluid. Numberless gradations are placed between these; so that, though the two ends of the chain are immeasurably remote, there is close approximation between any two links."

"Take any organ, or system of organs, and the same progress from complication to simplicity will be apparent. Let us observe the nervous system. In man and the mammalia, this apparatus consists of a brain and spinal marrow, securely lodged in bony cases; of cerebral and spinal nerves; of the system of ganglia, called the great sympathetic nerve, and of the five external senses. In passing through the mammalia, we find the brain considerably reduced in size; still farther diminished and altered in its figure and component parts in birds; lessened and greatly simplified again in reptiles and fishes."

"In the mollusca this large apparatus is reduced to one or more small ganglia, with a few slender nerves; to which are added the rudiments of an ear in one instance only, and in some others imperfect and almost doubtful organ of vision."

"In articulated animals there is merely a straight cord with a few branches; in some of the more complicated radiated animals, a few almost doubtful nervous branches; and below them nothing, neither brain, ganglia, nerves, nor organs of sense."

"But there would be little inducement to compare together the

various animal structures, to follow any apparatus through the whole animal series, unless the structure were a measure and criterion of the function. Just in the same proportion as organization is reduced, life is reduced; exactly as the organic parts are diminished in number and simplified, the vital phenonema become fewer and more simple, and each function ends when the organ ceases. This is true throughout zoology; there is no exception in behalf of any vital manifestations."

"The same kind of facts, the same reasoning, the same sort of evidence which shews digestion to be the function of the stomach, alimentary canal, motion of the muscles, the various secretions of their respective glands, prove that sensation, perception, memory, judgment, reasoning, thought, in a word all the manifestations called mental or intellectual, are the animal functions of their appropriate apparatus, the central organ of the nervous system. No difficulty, nor obscurity belongs to the latter case which does not equally affect all the former instances; no kind of evidence connects the living processes with the material instruments in the one which does not apply just as clearly and strictly to the other. Let us survey the natural history of the human mind;—its rise, progress, various fates and decay;—and then judge whether these accord best with the hypothesis of an immaterial agent, or with the plain dictates of common sense, and the analogy of every other organ and function throughout the boundless extent of living beings."

"The number and kind of intellectual phenomena in different beings correspond closely to the degree of developement of the brain. The mind of the Negro and Hottentot, of the Calmuck and Carib, is inferior to the European; and their organization is also less perfect. The large cranium and high forehead of the Orang-outang lift him above his brother monkeys, but the developement of his cerebral hemispheres, and his mental manifestation are both equally below those of the Negro. The gradation of organization and of mind passes through the monkey, dog, elephant, horse, to other quadrupeds; thence to birds, reptiles, and fishes, and so on to the lowest links of the animal chain. As the structure of the brain is more exquisite, perfect and complete, its functions should be proportionally so. It is no slight proof of the



doctrine now enforced, that the fact is actually thus; that the mental powers of brutes, as far as we can see, are proportional to their organization." (p. 100, 3, 4, 5, 6, 8, 9.)

However men may differ from Mr. Lawrence in the inferences they draw, we suppose all will admit the correctness of his facts. That mind must be a dull one indeed, which has not sometimes turned its attention upon itself and speculated on its own "origin, various fates and decay," and which in doing this, has not found every thing to excite its admiration, and much that is totally inexplicable by its finite powers. The relation between cerebral organization and thought is unquestionably intimate. That there is a constant action and reaction of the one upon the other cannot be denied. But to say "that cerebral, or medullary matter can remember, judge, and reason," and, to follow this idea to the consequence to which Mr. L. seems inclined, that when this organization is destroyed, all these faculties at the same moment cease to exist, is what we cannot bring our minds at all to believe. It seems to us, that any one who will for a moment turn his thoughts upon the operations of his own mind, must feel conscious that if the limbs be lopt off, the trunk with its various organs destroyed, or even if that beautiful structure, which has been considered more particularly the seat of thought, be broken up, still there will be something left. It is this intelligent something which in man constitutes individuality and personal identity. It is consciousness, thought, moral affection, and knowledge which is the essential part of intelligent beings; and which alone are necessary to constitute personal identity and moral responsibility. Now we conceive that this is not a mere quality of organized matter, but a complete entity, and for aught that appears to the contrary, this conscious being may have a separate and independent existence, and may continue to improve by its own powers, after the body, with which it was connected, having performed the office to which it was destined, has returned to the chemical elements of which it was composed. We conceive that every man must be conscious that all which can distinctly and emphatically be called self, is this intelligent something, and nothing else; and that every other part may in some sort be considered adventitious, and con-

tingent, and capable of being alienated, without its identity being in any degree impaired. Yet it cannot be denied that it is difficult to explain the facts stated by Mr. Lawrence, and a thousand others analogous to them, which meet us on every side, without acknowledging that this consciousness and thought may be derived from a living organized body, and, during life, intimately connected with it. We conceive, however, that even this may be conceded without our abandoning a point of the sublime doctrine of the immortality of the soul, and of its separate and self-existing power after the dissolution of the body. There seems to us nothing contrary to reason or experience in supposing, that even if this be derived from living organized matter, yet that after it has been once formed, it may still continue to exist after that from which it received its existence has ceased to be.

Perhaps it may be necessary to make some apology for taking up so much time in the discussion of a subject which may, in some degree, be considered foreign to the object of our journal. Our excuse is, that we found it necessary to state the views of Mr. Lawrence on this subject, because they may be considered as the foundation of the reasonings found in the remainder of the work; nor could we refrain from presenting at the same time, our own impressions on this interesting question.

The next subject taken up by Mr. Lawrence, and to which by far the greater portion of the lectures are devoted, is the natural history of man. He remarks, very justly, that it is only of late years, and principally through the exertions of Blumenbach, that public attention has been turned to this interesting inquiry, than which we conceive there is nothing more instructing or delightful. The animals which approach nearest to man in their anatomical structure, are the monkey family. These resemble him so nearly that they have received from this circumstance the appellation of "*anthropomorphous*." Of these the *simia satyrus*, and the *simia troglodytes* are the most strongly characterized in this respect.

"The *simia satyrus* is the true animal so much celebrated under the name of orang-outang. It is principally, if not solely, found in the island of Borneo. It is about three feet high; as the spe-



cimens conveyed hither were young, we may suppose that it would reach between three and four feet when grown up, but none have been seen in Europe exceeding three feet. The body is covered with strong reddish brown hair. The front of the head has a very human character, the forehead being large and high, and the facial angle consequently considerable: indeed no animal approaches so nearly to man as this in the form of the head or the volume of the brain. The face is bluish or lead-coloured; there are no cheek pouches or callosities of the buttocks. Two large membranous bags cover the front of the neck under the skin, and open into the larynx between the os hyoides and thyroid cartilage, a structure which spoils him for speaking. The thumb of the hind hand has no nail. It is a mild, and gentle animal, with some actions similar to ours, and some appearances of human feelings. It soon becomes attached, and imitates very quickly whatever we do. A state of captivity in climates and with a diet unfriendly to its nature, is not well calculated to develope its feelings and powers, or lead to a just estimate of its faculties and intelligence." p. 129.

"The *simia troglodydes* is a native of Angola and Congo, where it is called by the natives chimpanzé. It resembles the former in size, but differs from it by being covered by black hair, in having a lower forehead and large ears, and nails on the thumbs of the hind hands. It is very susceptible of education, and quickly learns to imitate human actions." p. 132.

Instead of placing him as other naturalists have done, among the mammalia, Mr. Lawrence observes, p. 132—"The peculiar characteristics of man appear to me so very strong that I not only deem him a distinct species, but also put him into a separate order by himself. His physical and moral attributes place him at a much greater distance from all other orders of mammalia, than those are from each other respectively. The zoological statement of his principal characters follows."

"Order bimanum (two-handed): genus *homo*; the species single, with several varieties hereafter enumerated.

Characters; erect stature, two hands, teeth approximated and of equal length, the inferior incisors perpendicular; prominent chin; rational; endowed with speech; unarmed; defenceless."

1st. Of his erect posture: Mr. Lawrence shows from anatomical phenomena, that though many other animals may be disciplined so as to remain in this position for a considerable time, yet from the peculiar structure of his body, man is the only one to whom it is natural.

“The length and strength of the lower limbs, the great instruments of support and progression, are very striking and quite peculiar to man. They are equal in length to the trunk and head together, which is not true in any other animal excepting the kangaroo and jerboa.” In all the monkey tribe they fall very far short of this proportion; even in the orang-outang they are short and weak, and manifestly inadequate to support the body erect. P. 142. “The line of direction in the human femur is the same as that of the trunk, but in all other animals it forms an angle with the spine, often an acute one.”

“The feet, the ultimate supports of the whole frame, and the primary agents of locomotion, are characterized by a combination of greater breadth, strength and solidity, in proportion to the size of the body, than those of any animal. The whole surface of the tarsus, metatarsus and end toes rests on the ground, and the os calcis forms a right angle with the leg. The two last circumstances are seen in no other animal; even the simiæ and bear have the end of the os calcis raised, so that this bone begins to form an acute angle with the leg. The dog, cat, and other digitated quadrupeds, even the elephant himself, do not rest on the tarsus, or carpus, but merely on the toes; the cloven-footed ruminants, (bisulca) and the solipeda touch the ground merely with the extremities of the third phalanges, and the os calcis is raised nearly into a perpendicular position.” “This single bone is therefore an infallible characteristic of man; and ‘*ex calce hominem*,’ would probably be a safer rule than ‘*ex pede Herculem*.’” p. 144.

“The peculiarities of the pelvis coincide with those of the lower limbs. The form of this part is very characteristic in man, and distinguishes him from the simiæ and indeed all other mammalia.” “The distribution, size, and offices of the muscular masses correspond to the organic arrangements of the skeleton. The lateral and posterior surfaces of the pelvis give origin to the powerful glutei, of which the exterior, exceeding in size all the other



muscles in the body, and covered by a remarkable stratum of fat, form the buttocks, which by their ample, fleshy, and convex protuberance conceal the anus; and are accounted by the classical authors in natural history, as Aristotle and Buffon, and by the greatest physiologists, as Galen and Haller, as the chief characters by which man is distinguished from the buttockless simiæ." "In standing on both feet, the *glutei magni* fit the pelvis firmly behind, and counteract the natural tendency to fall forwards, which with the weight of the head, the usual position of the upper limbs in front of the body, and the prominence of the abdominal viscera impress upon the trunk. Hence the bulk and power of these very muscles in the human subject, afford a clear proof that man was designed for the attitude on two feet. This muscle is so small in animals that it may be almost said not to exist." "The exterior muscles of the ankle joint, and chiefly those which form the calf of the leg, are the principal agents in progression. Hence man is particularly characterized by the largeness of his calves, and no animal equals him in this respect."

"The whole arrangement of the thorax corresponds to the erect attitude of man. It is flattened anteriorly, possesses a very broad sternum, is wide transversely, but shallow from before backwards." This, together with the long clavicles attached to the sternum, throw the shoulders apart and give an extensive range to the motions of the superior extremities. On the other hand, "quadrupeds have a thorax compressed laterally, narrow and keel-shaped on its sternal aspect, consequently deep from sternum to spine. This structure, with the absence of the clavicles, allows the front legs to come near together, to fall perpendicularly under the front of the trunk, and support it with firmness and facility." p. 150.

There is a strong analogy between the structure of the superior and inferior extremities. On examination, however, a great difference may be observed arising from the function which they are destined to perform. "The human hand being terminated by long and flexible members, of which only a small portion is covered by the flat nails, while the rest, furnished with a highly organized and very sensible integument, form admirable organs of touch,

and instruments of prehension. The animal kingdom exhibits no corresponding part so advantageously constructed in these respects. At the same time the lateral attachment of the arms to the trunk, and the erect attitude give us the freest use of these admirable instruments." "The great strength of the thumb which can be brought into a state of apposition to the fingers, and is hence of the greatest use in enabling us to grasp spherical bodies, and take up any object in the hand, in giving a firm hold on any thing we seize, in executing all the mechanical processes of the arts, in writing, drawing, cutting, in short a thousand offices, which occur every moment of our lives, and which either could not be accomplished at all if the thumb were absent, or would require the concurrence of both hands instead of being done by one. Hence it has justly been described by Albinus as a second hand. '*Manus parva majori adjutrix.*'"

"All the simiæ possess hands, but the most distinguishing part the thumb, is slender, short and weak, even in the most anthropomorphous." "Some animals which have fingers sufficiently long and moveable for seizing and grasping objects, are obliged by the want of a separate thumb to hold them by means of the two fore paws, as the squirrel, rat, opossum, &c. Those which are moreover obliged to rest their fore feet on the ground, as the dog and cat, can only hold objects by fixing them between the paw and the ground. Lastly, such as have the fingers united by integuments, or inclosed in hoofs, lose all power of prehension." "Man is bimanus, and biped, or two-handed and two-footed. Monkeys, apes, and other anthropomorphous animals, can in fact be called neither bipeds nor quadrupeds; but they are quadrumanous, or four-handed. They have apposeable thumbs on the lower as well as the upper extremities, and thus their feet are instruments of prehension as well as their hands." We may now answer the question whether orang-outang and other simiæ, go erect or on all fours: they do neither, but live chiefly in trees, for which they are admirably adapted, having prehensile members, instruments for grasping and holding on both upper and lower extremities." "A very striking difference between man and all other animals consists in the relative proportions of the cranium and the



face; which are indicated in a general, but not very accurate manner by the facial line." "As the proportions of the cranium and face indicate those of the brain and principal external senses, and instruments of mastication, we shall not be surprised to find that they point out to us, in a great measure, the general characters of animals, the degree of instinct and docility they possess. Hence the study of these proportions is of high importance to the naturalist. Man combines by far the largest cranium with the smallest face, and animals deviate from these relations in proportion as they increase in stupidity and ferocity." "One of the most simple, though often insufficient methods of expressing the relative proportions of these parts, is by the course of the facial line, and the amount of the facial angle. Suppose a skull to be observed in profile, when the occipital condyles are at rest in the articular hollows of the atlas in the erect attitude of the body, and neither inclined forwards nor backwards, a line drawn from the greatest projection of the forehead to that of the upper maxillary bone follows the direction of the face, and is called the *facial line*; the angle which this forms with a second line continued horizontally backwards is the facial angle, and measures the relative prominence of the jaws and forehead." By some anatomists the facial angle is described as being formed by a line passing from the most anterior part of the forehead to the most anterior inferior prominence of the superior maxillary bone, and another line passing from this last point through the meatus auditorius externus. "In man alone is the face placed perpendicularly under the front of the cranium, so that the facial line is perpendicular, hence the angle thus formed approaches most nearly to a right angle in the human subject. The face of animals is placed in front of the cranium instead of under it; that cavity is so diminished in size that its anterior expanded portion or forehead is soon lost as we recede from man. Hence the facial line is oblique, and the facial angle acute, and it becomes more and more so as we descend in the scale from man. In several birds, most reptiles, and fishes, it is lost altogether, as the cranium and face are completely on a level, and form parts of one horizontal line." p. 168.

"The invaluable remains of Grecian art show that the ancients

were well acquainted with these circumstances. They were aware that an elevated facial angle, produced by a great development of the instrument of knowledge and reflection, and a corresponding contraction of the mouth, jaws, tongue, nose, indicated a noble and generous nature. Hence they have extended the facial angle to  $90^\circ$  in the representation of legislators, sages, poets and others, on whom they wished to bestow the most august character. In the statues of their heroes and gods, they have still further exaggerated the human, and reduced the animal characteristics, extending the forehead over the face, so as to push the facial line beyond the perpendicular, and to make the angle  $100^\circ$ ."

"The facial angle in the human subject varies from  $65^\circ$  to  $85^\circ$ ; speaking of the adult (for in the child it reaches to  $90^\circ$ ). The former is a near approach to the monkey race." (Indeed in some of the simiæ the facial has been found to be  $65^\circ$ .) "Perhaps the angle may have extended beyond  $85^\circ$ , as the Greeks have done in their representation of Deity. However,  $100^\circ$  seems to be the *ne plus ultra*, beyond which the proportions of the head would appear deformed. That angle, according to Camper, constitutes the most beautiful countenance, and hence he supposes the Greeks adopted it. For, says he, it is certain no such head was ever met with; and I cannot conceive any such should have occurred among the Greeks, since neither the Egyptians, from whom they probably descended, nor the Persians, nor the Greeks themselves ever exhibit such a formation on their medals when they are representing the portrait of any real character. Hence the ancient model of beauty does not exist in nature, but is a thing of imaginary creation. It is what Winkemann calls the '*beau ideal*.'" p. 170.

The completely arbitrary nature of taste has long been proverbial, but we cannot allow so good an opportunity of illustrating the truth of the proverb as now offers in comparing the *beau ideal* of the Greeks with that of the Caribs. We think too, the comparison is a very happy comment on the justness of the doctrines which have been laid down. "There is no American in which the forehead is so low as in the Caribs. In order to exaggerate a character which they deem beautiful, they have had recourse to



artificial means for flattening this region at the time when the bones are soft and capable of yielding to artificial pressure." It is observed by Humboldt, that "the custom of flattening the head had its origin in the idea that beauty consists in such a form of the frontal bone as to characterize the race in a decided manner. The Antees who never disfigure the heads of their children, represent their principal divinities, as their hieroglyphical manuscripts prove, with a head much more flattened than any I have seen among the Caribs." p. 370.

"The vast superiority of man over other animals, led physiologists at a very early period to seek for some corresponding difference in the brains of man and animals."

"It has been asserted from remote times, that the brain of man is larger than that of any animal." There are certainly but few exceptions to this general rule, as may be easily shown "by contrasting the compressed, narrow, elongated crania of brutes, hidden behind their enormous jaws and face, with the length, breadth, and ample vault of the human '*cerebri tabernaculum*,' whose capacious and globular expanse surmounts and covers the inconsiderable receptacle of the senses and alimentary apparatus." p. 190.

"In later times the subject has been investigated in a different way; by comparing the proportion which the mass of the brain bears to the whole body. The result of this comparison in the more common and domestic animals was deemed so satisfactory that, without prosecuting the inquiry further, a general proposition was laid down that man has the largest brain, in proportion to his body. More modern physiologists, however, in following up this comparison, have been perplexed at discovering many exceptions to the general position. They found that several mammalia, as the dolphin, seals, some quadrumana, and some animals of the mouse kind, equal the human subject, and that some small birds even exceed him in this respect."

These observations being overturned, Sæmmering has proposed another point of comparison, viz.—"Let us divide the brain into two parts; that which is immediately connected with the sensorial extremities of the nerves, which receives their impressions, and is therefore devoted to those common wants and pur-

poses which we partake with animals. The second division will include the rest of the brain, which may be considered the seat of the mental phenomena." "In proportion then as any animal possesses a larger share of the latter and more noble parts; that is, in proportion as the organ of reflection exceeds that of the external senses may we expect to find the powers of the mind more diversified, and more fully developed. In this point of view, man is decidedly pre-eminent. Although in his senses and common animal properties he holds only a middle rank; here, he surpasses all other animals that have been hitherto investigated."

Sœmmering, remarks—"Animals of various kinds seem to me to possess a larger or smaller quantity of this superabundant portion of brain, according to the degree of their sagacity, or docility. The largest brain of a horse which I possess, weighs one pound seven ounces; the smallest human brain that I have met with in an adult, two pounds five ounces and a quarter. But the nerves in the basis of the horse's brain, are ten times larger than in the other instance, although it weighs less by fourteen ounces and a quarter. But we are not hastily to conclude, that the human species have smaller nerves than any other animals. In order that my ideas may be understood, I shall state the following imaginary case. Suppose the ball of the eye to require 600 nervous fibrils in one instance, and in another half the size 300; further, that the animal with 600 fibrils possesses a brain weighing seven drachms, and that with 300 a brain only weighing five drachms. To the latter we ought to ascribe the larger brain, and a more ample capacity of registering the impressions made on the organ of vision. For allowing one drachm of encephalon to 100 fibrils, the brain which is absolutely the least, will have an overplus of two drachms, while the larger has only one." This idea of Sœmmering is certainly very ingenious, though not without objection. The assumption on which it is founded, that "a certain bulk of nerve requires always the same proportion of brain for the execution of its office," as is justly observed by Mr. Lawrence, is by no means self-evident.

"The most striking character of the human brain, is the prodigious developement of the cerebral hemisphere, to which no



animal affords any parallel. It is also the most perfect in the number and developement of its parts; none being found in any animal which man has not, while several of those found in man, are either reduced in size or deficient in various animals." p. 195.

Some travellers have pretended that there are some of the simiæ which are destitute of a hairy integument, and others again that there were races of men whose skin were covered with hair. These are however, mere fables, and clearly proved not to be entitled to belief. "The smoothness and nakedness of the human integuments therefore, form a sufficient diagnostic character of our species, as compared to the monkey, or any other allied mamiferous animal." p. 186.

Man is also distinguished for the flexibility of his physical constitution, and its capability of enduring all possible degrees of atmospherical heat and cold. The hymen has been supposed by many physiologists to be peculiar to the human subject, and some inferences of a moral character have been drawn from this assumed fact. It appears however, from the evidence of Cuvier, that several mammalia have a distinct membranous fold at the entrance of the vagina, and others a decided contraction in the same situation.

Almost all animals have some particular class of substances as aliment, and their digestive powers are only capable of dissolving these substances. They are therefore usually divided into carnivorous and herbivorous; from their habits in this respect, while others again subsist entirely on some individual article of these substances. The formation of the teeth is considered a very just index of the habits of the animal. "The nature of an animal is only to be learned by an observation of structure, actions, and habits. From the powerful fangs and jaws, the tremendous talons, the courage, and the vast muscular strength of the lion, and his constant practice of attacking live prey, we pronounce his nature to be ferocious, predatory and carnivorous. From evidences of the same sort, we determine the nature of the hare to be mild, timid and herbivorous. In a similar way, we conclude man to be naturally omnivorous; finding that he has instruments capable of procuring, masticating and digesting all descriptions of food, and

that he can subsist in health and strength on flesh, or vegetables only, or on a mixture of both." P. 211. But the digestive organs of man, have not only the power of drawing support from the animal and vegetable kingdoms, but it appears from the reports of travellers, that the mineral kingdom is made tributary to their wants in this respect. "The Ottomaques, says Humboldt, on the banks of the Meta and the Oronoco, feed on a fat unctuous earth, or a species of pipe clay tinged with a little oxid of iron. They collect this clay very carefully, distinguishing it by the taste; they knead it into balls four or six inches in diameter, which they bake slowly before a slow fire. Whole stacks of such provisions are seen piled up in the huts. These clods are soaked in water, when about to be used; and each individual eats about a pound of the material every day. The only addition which they make to this unnatural fare, consists in small fish, lizards, and fern roots. The quantity of clay that the Ottomaques consume, and the greediness with which they devour it, seem to prove that it does more than merely distend their stomach, and that the organs of digestion have the power of converting from it something convertible into animal substance." The same practice has been observed in other places.

P. 237. "I consider the differences between man and animals in propensities, feelings, and intellectual faculties, to be the result of the same cause, as that which we assign for the variations in other functions, viz: difference of organization; and the superiority of man in rational endowments, is not greater than the exquisite, complicated, and perfectly developed structure of his brain, and particularly of his ample cerebral hemispheres, to which the rest of the animal kingdom offers no parallel. That the senses of man, and other animals, will not explain all their varied and wonderful mental phenomena, and that the superiority of man can by no means be deduced from any pre-eminence in this part of his construction, are truths too obvious to require further notice."

"Some modern inquirers have gone beyond this general statement, and have ventured to particularize in the brains of animals and of man the organ or residence of each propensity, feeling



and intellectual power. I cannot pronounce on the accuracy and completeness of the mental and cerebral survey executed by Messrs. Gall and Spurzeim; nor pretend to judge of the exactness and fidelity with which the numerous positions are marked down in their very complete and well filled map of the brain. They appeal to observation for the confirmation or refutation of their statements; but my observations are not numerous or varied enough for these purposes, nor can we refuse them the merit of patient inquiry, careful observation, and unprejudiced reflection. They have performed the useful service of rescuing us from the trammels of doctrines and authorities, and directing our attention to nature. Her instructions cannot deceive us. Whether the views of Gall and Spurzeim be true or not, our labours in this direction must be productive."

It is so seldom that any thing relating to our country is complimented, or even treated with common civility by literary or scientific Europeans, that when we meet with praise it falls upon our ear like the unexpected sound of water in the desert. We are desirous of interesting our readers in Mr. Lawrence, and we are sure to some of them at least, the liberality and respect with which he speaks of our country, its institutions and illustrious men, will not be considered the most insignificant nor least inviting part of these lectures. We shall make a short extract as a specimen of his manner of speaking of America. We take the first we meet with of this kind.

"Le Suer,\* the fellow traveller of Peron, who had long promised a natural history of the medusæ, to be illustrated by those inimitable delineations which he brought back from their voyage of discovery to the Austral regions, has found himself unable to complete this undertaking, and is gone, with many others to the new world. If we cannot repress a sigh when we see men of peaceful pursuits thus torn from their native soil, and driven into

\* We are happy to have it in our power to do justice to an amiable and distinguished man, who has chosen this country for the exertion of his eminent talents. We have great pleasure in enabling our readers to judge of the high rank he held in Europe, because we fear his talents are not sufficiently appreciated in this city.

foreign climes, let us rejoice not only for them, but for all mankind, that such an asylum for the victims of power and oppression exists: that there is not a spot, but a vast region of the earth, lavishly endowed with nature's fairest gifts, and exhibiting at the same time the grand and animating spectacle of a country sacred to civil liberty. The numerous people whose happiness and tranquillity are so effectually secured by the simple forms of a free government are the growth of yesterday; at the same rate of progress, they may reach in our lives as gigantic a superiority over the worn out despotisms of the old world, as the physical features of America, her colossal mountains, her mighty rivers, her forests and her lakes exhibit in comparison with those of Europe." p. 37.

We must now hasten to bring our agreeable task to a close. We have been induced to make pretty copious extracts from this book because of the interesting nature of its contents, and because of the difficulty of obtaining access to it, there being few, perhaps no other copy in the country, than the one in our possession. But copious as these extracts are, we are conscious that they will convey but a very inadequate idea of the excellence of the work itself.

We have thus finished for the present our account of this delightful volume, an expression, by the way, which is seldom applicable to this department of our duty. Our path for the most part lays through regions which may be productive, but are seldom very beautiful, or inviting. It is refreshing sometimes to meet with occasions, when we may turn aside from the more dry and recondite pursuits of the profession, and luxuriate for a time amidst these more agreeable and interesting topics; to leave those scenes where we witness only the miseries of our fellow creatures, and those circumstances which are humiliating to our pride, and ponder on that most wonderful of all the products of divine wisdom, man,—his exalted powers and noble destiny; to speculate on his moral and physical nature, and the relations in which he stands to that infinite variety of objects which surround him on every side. We propose in another number to take up this book again, and examine the opinions of the author "on the varieties of the



human species," a subject which occupies more than half the lectures.

Although this work can by no means be said to be destitute of faults, yet as we have already spoken with considerable asperity of by far the most crying sin of the author, and as the faults are redeemed by so much merit, we shall spare ourselves any further exercise of this most ungrateful part of our duty. The topics discussed, are undoubtedly among the most curious and interesting which can occupy the human mind, and are certainly managed with great ingenuity and ability. We think the work exhibits the strongest internal evidence of intellectual vigour and profound research. We cannot close these observations without expressing our thanks, and again regretting a circumstance which has deprived the author of the full measure of fame to which he is entitled, and the public of a work replete with instruction and amusement.

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ART. XI. *Review of an Essay entitled "Thoughts on the Pathology and Treatment of Cynanche Trachealis, or Croup. By N. CHAPMAN, M. D."*

[Philadelphia Journal of the Medical and Physical Sciences, p. 299.]

WE have always been under the impression, that the credit of having originally noticed and described croup, was justly accorded to professor Home of Edinburgh, whose publication appeared in 1765. It would appear from the essay before us, that Dr. Chapman, in turning over one of the earliest volumes of the Transactions of the Royal Society of London, found a very distinct account of the disease, illustrated by dissections. The writer is stated to have been an obscure practitioner, and from what we can gather, probably of Cornwall, in England. We would have gratefully acknowledged our obligations to the erudite professor of the practice, had he only kindly favoured us with the name of this humble son of Esculapius, or had he but indicated the volume from which he drew his information. Peculiarly felicitous in his modes of acquiring knowledge, Dr. Chapman seems to be

totally unmindful of the long and toilsome task imposed on those, who wish to follow him in exploring the archives of medical science.

With the slender information obtained from Dr. Chapman's meagre reference, we have searched the Transactions of the Royal Society of London, and found "an account of the morbus strangulatorius, communicated in a letter from John Starr, M. D. to John Mortelber, M. D. Sec. R. S." The letter is dated \*Liskard, January 1<sup>st</sup> 1749, and was read before the society, May 24, 1750.

From a careful perusal of the paper, we are induced to believe, that the morbus strangulatorius was merely an aggravated form of cynanche maligna; it certainly did not present the symptoms which are considered as characterising idiopathic croup. In the case, which is stated by Dr. Starr, as most satisfactorily exhibiting the nature of the disease, we are informed nearly in the following terms:

"The first complaints were a slight pain in swallowing: a hoarse vexatious cough like that of an incipient catarrh, accompanied by pain shooting through the ears. There was likewise a copious ichorous discharge from the mouth, supposed to amount to a quart or three pints in the course of a day. On examining the fauces and depressing the tongue, a white substance was seen covering the velum pendulum palati and the tonsils. The surrounding parts were of a somewhat deeper red than natural. His breath stinking and highly offensive."

After occasionally gargling and rubbing the part, with a probe covered with cotton dipped in a mixture containing muriatic acid, in a violent fit of coughing, the membrane separated from the velum palati. In the engraving, which accompanies the paper, the membrane is represented in length about four inches, and in its greatest breadth, about an inch. Its figure, moreover, corresponds to that of the parts which it is stated to have covered.

Some days after, the patient's father perceiving something in

\* A town in Cornwall; is one of the four stannaries, and sends two members to parliament.



the lad's mouth which he thought thick phlegm, thrust in his finger and thumb, and taking hold of it, drew it out. Dr. Starr describes it in such terms, as to leave no doubt that it was an exudation of coagulable lymph thrown out from the internal membrane of the trachea and bronchial tubes. This substance is likewise represented in the engraving, which we have noticed as accompanying the memoir.

The boy ultimately died; but no examination was made after death, nor could we discover that the history was illustrated by dissections.

If this statement has any tendency, unjustly, to impair confidence reposed in the correctness of Dr. Chapman's readings and quotations, we have our apology in the unnecessary obscurity in which he retains the name of the author, whose claims he so anxiously urges in opposition to those of the Edinburgh professor.

Croup is supposed to be confined to the early period of life, and is known to affect chiefly children of robust and florid complexion. When it attacks those of a delicate complexion, it is ascribed to peculiar idiosyncrasy. The observations are fully confirmed by the experience of Dr. Chapman.

None are ignorant of the changes which the human system undergoes at the age of puberty. In males particularly the larynx is altered both in figure and dimensions, and the coincidence of these circumstances with the change of voice is familiar to every one. It is, however, not so evident, that "debility and relaxation" are necessary attendants on the condition of the larynx during the period of childhood, and therefore "inviting to morbid aggressions."

Medicine, though not to be ranked among the exact sciences, requires a precision and sobriety of expression that will not admit of the substitution of mere words, as cause and effect, in the explanation of the various phenomena of morbid affections. When the teacher, or the writer is ignorant, let him not disguise it; for in proportion as we advance in knowledge, are we made sensible of how much we have to learn. The deep tones of voice characterising manhood, do not indicate to us the state of the membrane lining the larynx, nor do they demonstrate, that the parts constituting the seat of croup have undergone a change which enables

them to resist the exciting causes. We have no hesitation in thus animadverting on this theory, for it is of transatlantic origin, and belongs to Cheyne; whose anatomical knowledge and pathological observations ought to have afforded him an explanation more consonant to the actual structure of the parts, and to the known laws of the animal economy.

The greater prevalence of trachitis among children may, according to Albers, admit of the following explanation. In the first place, they are more predisposed to inflammation of the mucous membranes; moreover, when the disease is connected with the exanthemata, it is observed to be peculiar to persons of a tender age. But the cause on which the greatest stress may be laid, is the state of the rima glottidis, this aperture being more than proportionably small in persons under the age of puberty. To which ought to be added their more frequent exposure to the exciting causes. Those under five years of age, the period of life most obnoxious to the disease, either from the restraints imposed on them, or from the want of incitements to active exercise, are often observed, in <sup>heavy</sup> and inclement weather, with their necks and bosoms bare, sitting at the portals of our doors, their attention so engrossed with passing objects, that they are totally insensible of their perilous situation. Under such circumstances, who can be surprised at their being more frequently affected with croup?

The majority of practitioners and our best medical writers, have considered croup as an inflammatory affection; nor can the suddenness of the attack be looked upon as tending in the least to invalidate this opinion. In parts that fall directly under our inspection, as the fauces, how often do we see considerable tumefaction, straightening the passage, come on in the course of a short period after exposure to cold. The difficult deglutition, the painful sensation in the throat, together with the other symptoms characterising cynanche tonsillaris do, moreover, frequently disappear in the course of a night; and the patient awakens, after a refreshing sleep of a few hours, without the slightest vestige of the disease. The inflammation that begins in the mucous membrane lining the larynx may, indeed, be communicated to the subjacent muscles, and then these will perform their office irre-



gularly and with pain. But such affection could not be denominated spasmodic croup.

Of the existence of spasmodic croup, and of the means by which, in case of its existence, it may be distinguished from the inflammatory affection, the perusal of page 301 of the Journal, will convince any one that Dr. Chapman entertains no very precise or accurate views. It is true, he waves the distinction to insist on the efficacy of blood-letting in the prompt reduction of spasm of high action; and the treatment of this disease is thus improperly anticipated, that he may have an opportunity of casting the censure of false and prejudicial practice on those who employ musk, asafoetida, and opium in combating the early symptoms of croup.

We now pass to the symptoms. These have been evidently delineated with a careless and hasty hand: they are badly grouped, and not presented in a regular and orderly succession. Before the eye has embraced the whole assemblage, the attention is most unpropitiously interrupted by the precipitate introduction of the mode of treatment.

Dr. Chapman announces his mode of managing the disease in terms sufficiently encouraging; it is stated to be "exceedingly simple, and has hitherto proved so successful that he always approaches it in the early stages with a greater certainty of curing it than any of the other complaints of infancy and childhood."

The bills of mortality in this city, exhibit the sad and melancholy list of 89 persons cut off by hives or croups, during the year which has lately passed. It is cheering, therefore, to be presented with the prospect, and the promise of seeing this number annually saved from the jaws of death; and who can forbear to indulge the pleasing anticipation of the addition that will thus be made to our growing population. But the physician who has watched over the ravages committed by disease, and witnessed how many persons are carried to an untimely grave, knows that a reasonable expectation of success must have its foundation in an accurate knowledge of the nature of the disease, and in a just conception of the means by which it may be brought to a happy issue. Of the pathological doctrines contained in the essay we have already expressed our opinion. With respect to the use of the remedies to be em-

ployed in the various stages of croup, the indications are not laid down with the precision required in combating a disease, considered by most practitioners as a highly obstinate, and dangerous affection.

The effervescence and hurry of thought, with the want of system, have occasionally betrayed the author of the essay into apparent contradictions that have not escaped the majority of his readers. In confirmation of this statement, we beg leave to offer a few extracts.

Having described what he is pleased to denominate spasmodic croup, he goes on to observe, in relation to the inflammatory affection: "In many cases the disease advances gradually with the ordinary catarrhal symptoms," &c. "Completely formed, there is no material difference between the two species of croup; and henceforward their progress is nearly, or perhaps exactly similar." p. 302.

"In the first or forming stage," he informs us, "that the disease is restricted pretty much to the upper portion of the trachea, and consists either in a spasmodic constriction of the glottis, or inflammation of the membranous lining of the larynx. But permitted to continue for ten or fifteen hours, and sometimes even a shorter interval, it extends itself to the bronchiæ and into the substance of the lungs, producing, sooner or later, vast collections of mucus and phlegm, or *exudation of coagulable lymph*, or an engorged state of the lungs." See pages 304-5.

Speaking of blood-letting, especially when carried ad *delequium animi*, he uses this expression: "When pushed to this extent, I may almost say venesection is invariably successful. As yet I have never known one instance in which it failed." p. 303.

Adverting to the existence of the adventitious membrane in the larynx, he observes, p. 310, "that it does occasionally exist cannot be denied, though I suspect rarely, as I never met with it in my repeated examinations for the purpose."

"The appearances I have observed in dissections relating to the larynx, were slight marks of inflammation with more or less of mucus, such as is formed by all the secretory surfaces. Why I have not seen the membranous productions is perhaps susceptible



of explanation. To throw out coagulable lymph, of which it is composed, requires the vessels to be excited, a state which by the copious depletion adopted in the cases that came under my notice was probably prevented."

From all which we learn, that whatever differences there may be in the attack, after a very short interval, spasmodic and inflammatory croup admit of no distinction. That in less than even ten hours the inflammation may extend into the bronchiæ, producing sooner or later an exudation of coagulable lymph; yet we are afterwards told, that, though he made repeated examinations for the purpose, he never could discover the membranous production, which he acknowledges to be composed merely of coagulable lymph. And why he never did meet with this appearance, we are very gravely informed, may be explained by his having employed copious depletion, thereby preventing that degree of inflammation which is required for the pouring out of coagulable lymph. But from his observations on blood-letting, it is evident, that if he had such frequent opportunities of dissecting ~~thae~~ those who had been under his care, it was *because he never could get them to faint!*

If Dr. Chapman is ambitious of sustaining the dignity of the chair to which he has been elevated, if he is desirous of advancing the medical literature of his country by his own productions, he must trim his midnight lamp, and devote his leisure hours to the careful perusal of our best authors, to the assiduous study of nature, to deep meditations on the objects connected with his profession.

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ART. XII. I. *Report of the Committee of the Medical Society of the City and County of New-York, explanatory of the Causes and Character of the Epidemic Fever, which prevailed in Bancker street and its vicinity, in the summer and autumn of 1820.* Published by order of the Society. New-York, 1820, pp. 38.

II. *Remarks on the Report of the Committee of the Medical Society of this City, on the "Epidemic Fever of Bancker street and its vicinity, in the summer and autumn of 1820."* New-York, 1820, pp. 16.

III. *A Statement of Facts relative to the late Fever which appeared in Bancker street and its vicinity.* Published by order of the Board of Health. New-York, 1821. pp. 28.

WE do not recollect to have ever met with a more audacious attempt to impose upon public credulity, than that which has lately been practised in New-York by a combination of men, who have been labouring with the utmost assiduity to propagate the belief, that during the past summer and autumn, the *yellow fever* has prevailed to a most alarming extent in that city.

In defiance of repeated and published assurances from the board of health; in defiance of the reports of the highest medical authorities of the city; in defiance of the testimony of some of the attending physicians of the cases of fever, as given before the board of health; in defiance of the fact that a day of solemn thanksgiving was set apart for the purpose of returning thanks to the Almighty, that amidst numerous other blessings he had preserved the city of New-York from the ravages of this pestilence; in short, in defiance of facts the most notorious and evidence the most irresistible, do seven men,\* viz: Samuel Osborn, Felix Pascalis, John Watts, Jr. John Neilson, Thomas Cock, Charles Drake, and Ansel W. Ives, come forward, and gravely tell us that all this is the result of a fatal delusion—that from the 21st of August to the 20th of October, no less than *one hundred and fifty* persons fell victims to the yellow fever (p. 30, of Report)—that in conse-

\* The committee who drew up the Report of the Medical Society.



quence of its prevalence the terrified inhabitants of the neighbourhood fled in numbers from their homes, p. 8; that of this state of things, the New-York board of health were duly informed, p. 28; that the board, although solicited to that effect, neglected to inquire properly into the character, &c. of this fever, and consequently neglected to adopt such measures as were calculated to arrest its progress, p. 28, 29; and finally, that this neglect arose from a conspiracy entered into between the board of health and the resident physician, p. 29.

From this statement it is obvious, that the subject matter of this report is of a very serious nature, involving very deeply the moral and professional-interests of a large number of respectable gentlemen; and as such, we conceive that it calls loudly for a rigid examination. If it shall appear, in reality, that the yellow fever has desolated New-York during the last season, and that the board of health of that city have not only most studiously and designedly concealed this fact from the public, but have at the same time refused to pursue those wholesome measures which have been sanctioned by time and experience, to protect their fellow-citizens from the ravages of this dreadful scourge; we shall feel it an imperative duty to unite with the framers of this Report, in holding them up to public odium as a body of men who have betrayed their sacred trust, and who have therefore forfeited every claim to public confidence and respect. But if, on the other hand, it shall be made evident that not a single case of yellow fever has occurred in New-York, during the last season, and that therefore the board of health were in no respect culpable for not guarding against an evil which had no existence;—if it shall be proved that the authors of this Report are the organs of a party who are personally and inveterately hostile to the resident physician, and that this report was designed to ruin the influence of that gentleman as a public officer; and moreover, if it shall appear that to accomplish this object, facts have been distorted or suppressed, and the board of health wilfully slandered, then we most sincerely hope, that the honest indignation of an injured and incensed community will light upon the heads of men capable of such desperate designs.

Having premised thus much, we shall now proceed to examine

the proofs which are brought forward in this report, to substantiate the position that the yellow fever has prevailed in New-York during the last year.

To do the committee justice, we readily admit that they have displayed no common zeal and industry in endeavouring to establish this point. Nevertheless, are we confident, that we are expressing the opinion of every unbiassed and candid person who has read this Report, when we say that they have completely and most disgracefully failed; and we have reason to think, that some of the committee themselves did not believe it.\* Indeed it appears very evident, that either the committee must have been very ignorant of the subject on which they were treating, or they must have set forth representations to the public which they knew to be unfounded, with the design of duping the public and injuring certain obnoxious individuals. Otherwise we are at a loss to account for the senseless absurdities and the stupid contradictions with which this Report abounds from the beginning to the end of it. To enable the reader to judge which of these alternatives is justly applicable to the framers of this Report, we shall enter into a brief analysis of the principal points embraced in this extraordinary document.

The Report begins with stating, that ample materials existed in Bancker street, for the generation of yellow fever; and here on the very threshold, in assigning the causes of the fever, we think the committee have been singularly unfortunate in the doctrines which they have advanced, inasmuch as they contradict the recorded experience of the best observers and ablest writers on this subject. In page 12 of the Report, they say, "your committee therefore feel authorised to infer that there has existed probably, adequate predisposing causes for the reproduction of those fevers,

\* "We must not be surprised to find those who write reports on this fever, (Bancker street fever) deny in conversation, that they believed it to be yellow fever, notwithstanding the reports themselves declare it to have been yellow fever, and that they have signed their names to such reports, or voted for their publication. *Four of the committee* itself are in this singular predicament, and have declared, in public, that the disease was not yellow fever!!!" Remarks on the Report of the committee of the Medical Society, &c. p. 16.



which are more peculiar to this climate, when assisted by impure miasmatic exhalations; *in the operation of the long continued and intense heat of the summer months, the stillness of the elements, the absence of the showers and thunder storms, with a continued cloudless sky, during long days of unabated and refracted heat.*" In confirmation of this, they tell us in another place, that "during the months of July, August and September, the atmosphere was more calm and dry than ordinary." p. 9. All this is very plain language, and there can be no mistake nor quibbling about its meaning. It is this; that the long continued and intense heat and *drought* of the season was one of the causes of the yellow fever. Now to us, this appears to be a very strange doctrine. From our acquaintance with the writers on this subject, we believe, that it was long ago agreed upon by all the advocates for the domestic origin of yellow fever, that heat, *independent of moisture*, was perfectly innocuous. On this point we shall refer to two or three authorities, whose opinions will doubtless be received with some deference by the authors of this Report. "Yellow fever," says Dr. Rush, "appears only in those climates and seasons of the year in which heat, acting upon *moist* animal and vegetable matters, fills the air with their putrid exhalations. *A vertical sun, pouring its beams for ages upon a dry soil; and swamps defended from the influence of the sun by extensive forests, have not, in a single instance, produced the disease.*" Medical Observations and Inquiries, vol. 4. p. 252-3. To the same effect, speaks Dr. Edward Miller. "The primary and essential cause of the yellow fever, is a miasma or pernicious exhalation floating in the atmosphere. In order to produce this miasma, it is necessary that there should be a concurrence of heat, *moisture* and a quantity of decaying animal and vegetable matter." Miller's Report.

But the most curious and satisfactory authority for our purpose is still in reserve. A writer who has given the public a history of the yellow fever as it prevailed in New-York in 1819, in accounting for the remarkable healthiness of the city previously to the appearance of the fever, gives us the following—"The second cause," says he, "of the continuation of public health, is

the long and remarkable drought in our city and island during the whole summer." "Large and populous cities," he adds, "are healthier with excessive and dry heat; had we been often refreshed by rains and showers, a calamitously sickly time might with probability have been expected in New-York."\* Our readers, we think, will be perfectly amazed when they are informed that the author of these sentiments is a prominent member of the committee who drew up the report under review. They cannot fail to admire the wonderful consistency of a man, who in one year publishes to the world, that nothing but the *absence of "rains and showers"* preserved New-York from "a calamitously sickly time," and in less than twelve months after flatly contradicts this, and then tells us that in 1820 the yellow fever is to be *attributed to "the absence of showers,* with a continual cloudless sky, during long days of unabated and refracted heat." Report, p. 9. A more flagrant specimen of contradiction cannot be met with in the records of inconsistency and absurdity. Let it not be objected, that an individual member of a committee is not responsible for every sentiment expressed in a report like this, which is the joint production of a number of persons. Even admitting this flimsy evasion to its fullest extent, Dr. Pascalis cannot plead this in exculpation of himself. For we know that he was one of the principal writers of the Report, and he defended it *in toto* before the Medical Society. But Dr. Pascalis is not the only member of this committee who has involved himself in so unpleasant a predicament. In a very heavy compilation† on the subject of yellow fever, published not six months before the appearance of this report, Dr. Watts expresses himself very much to the same effect with Dr. Pascalis, and in direct contradiction to the language of the Report. "The

\* See "a Statement of the Occurrences during a Malignant Yellow Fever, in the city of New-York in 1819, &c." by FELIX PASCALIS, M. D. p. 19.

† See "the Medical and Surgical Register of New-York, &c." Notwithstanding the opinion expressed by a certain reviewer, I must call this essay a heavy compilation. To those who have had the patience to read it, I need offer no proof of the correctness of this opinion. To those who have not seen it, I may just state that out of 156 pages about *sixty* full pages are literal quotations from other publications.



tendency of the weather" in 1819, says he, "to produce miasmatic diseases was supposed to be controlled by the *unusual dryness* of the season, being unfavourable to the production of those deleterious exhalations which usually engender a long list of summer and autumnal diseases; this perhaps is the reason in some measure, of our city escaping a greater degree of sickness."<sup>\*</sup>

But the committee, not satisfied with attributing the yellow fever to the absence of moisture, actually advance the monstrous doctrine that even *heat* is not necessary to generate that fever. That they may not be misunderstood we shall give their very words. "It is admitted, the causes of malignant (yellow) fever are not always assignable to *heat or cold, moisture or dryness*; but it is well known that in either of these states, putrefying vegetable and animal matter may produce malignant fever." Report, p. 8, 9. An opinion so much at war with fact and observation we shall not stop to refute, and indeed it seems quite unnecessary, inasmuch as it is contradicted in a few pages after, where we are told, that one of the "essential characteristics" of yellow fever, and by which it may be infallibly distinguished from typhus, is, that it is "expected *never* to break out, excepting *during, or immediately following the warmest season* of the year; and frost always stops its progress, and puts an end to its existence as an epidemic." Report, p. 16. How, we would ask, can this be the case, if it be true, as we are told, that yellow fever may be produced in a "state of *heat or cold*." We leave the committee, in their serious reflections on their production, to reconcile this if they can.

Putrefying animal and vegetable materials, is another cause assigned in this report, as giving rise to yellow fever. Admitting, for the sake of argument, that this *may* produce yellow fever, still we think, the committee are very far from having proved that it existed in Bancker street. From any thing that is shown in the Report, it does not appear that any extraordinary collections of animal or vegetable materials existed in that district. The amount of what the Report tells, is, that in Bancker street, a very dense population of paupers were crowded in small and confined

\* Medical and Surgical Register, p. 290.

apartments, in which as we think, there was no more filth than would have necessarily resulted under such circumstances in any part of the world. And what shows beyond a doubt that no such accumulations of animal or vegetable matters could have existed there, is the fact, which is repeatedly mentioned in the report, that a large number of hogs were running loose in the yards, who would of course have speedily consumed all such materials. In confirmation of this opinion, it may not be improper to remind the reader, that so deeply were the physicians of Baltimore convinced of the utility of hogs in this respect, that they recommended the corporation of that city to suffer these animals to run at large through the streets. But, besides, where could such an abundance of animal and vegetable matter have come from, as would have been sufficient to generate yellow fever? The people were poor; and their food was scanty; and it is not to be supposed, that under such circumstances much would be wasted or thrown away. We might push our argument much further on this point, but we have said enough to prove the absurdity of the supposition that large masses of putrescent animal and vegetable matters were collected in Bancker street, and we have chosen this method of refutation, in preference to that of offering the positive testimony of impartial witnesses to contradict the statements of the report, from a feeling of delicacy towards the members of the committee; we hope they may have wisdom enough to appreciate our motives, and not take any advantage of our lenity. We have thus glanced at some of the causes assigned as having given birth to the fever of Bancker street, and as we think, have shown the statements of the committee to be absurd and contradictory.

The committee next proceeds to show that a malignant constitution of the atmosphere prevailed during the summer and autumn, and that this co-operated with the causes before mentioned in the production of the fever, and here again we have another signal instance of downright contradiction. In proof of the malign condition of the atmosphere, we are told that "remitting fever, cholera, and dysentery, prevailed to some extent, and caused considerable mortality, especially among children;" that "*in various*



parts of the city, remitting fevers of more than ordinary severity occurred, and in many instances assumed a character of malignity;" that "as early as the latter part of July, there occurred several cases of sudden death from fever, which occasioned the suspicion of the commencement of a fever of a malignant type;" that "an unusual number of febrile diseases appeared in the city prison and Bridewell in July and August;" that "typhus gravior prevailed at the hospital at Bellevue," pp. 9, 10, 11. And yet notwithstanding all this detail, we are seriously told that the "summer and autumn elapsed, however, without any very remarkable degree of mortality in the city generally," p. 9. Which of these statements are we to believe? The committee cannot be quite so unreasonable as to require us to believe both. This would really be imposing too much upon good nature, though we are aware that the framers of this report have calculated very largely upon the credulity of the public. But it is a matter of very little consequence, as it regards the character of the gentlemen concerned, which we admit to be true. If it be true that there was no "extraordinary mortality" in the city during the last season; then away go all the arguments and facts which they have adduced to prove the malignant constitution of the atmosphere. If, on the other hand, it is true, as they endeavour to prove, that the city was uncommonly sickly, and that diseases were remarkably mortal, what becomes of their consistency and veracity? We regret that we have been obliged to push the committee into this dilemma, and we shall leave them to extricate themselves at their leisure.

We cannot take leave of this part of the Report without quoting one of the most *admirable* specimens of subtle argumentation, that we have ever met with. It can only be equalled by the *classical and vigorous* language in which it is couched. Here it follows *verbatim* and *literatim*. "To illustrate the pre-existing causes of malignant fever, your committee notice the case of a black boy, whose great yellowness of the eyes, and other appearances [what appearances?] induced a magistrate to order the examination of the corpse, which, after all, offered only what the physician had

previously pronounced [what? yellow fever, or the plague? oh no!] a violent acute peripneumonia." Report, p. 11. This is most exquisite, and if any person can read it without having his risible faculties called into exercise, we shall call him a very Heraclitus. *Risum teneatis amici?* We should like to know in what manner this *elegant* paragraph was composed. It is certainly too good to have been the work of a single person. We suspect that it is the product of the combined wit and skill of the whole committee. But to be serious: how a black boy's dying of an acute peripneumonia, a disease of every day occurrence, can prove a malignant constitution of the atmosphere, we confess ourselves at a loss to determine. That *superior wisdom*, however, which first promulgated it, can no doubt assign a satisfactory explanation of this as well as other difficulties, and we shall look with great anxiety for a second edition of the Report accompanied with notes and illustrations.

We fear we have spent too much time upon this part of the Report, and we hasten to something more important; we mean the opinion of the committee concerning the *character* of the fever in Bancker street.

"Respecting the nature of the Bancker street fever, your committee are aware of many difficulties in the way of discrimination; owing to some peculiar circumstances connected with the disease, and because medical gentlemen have promulgated, perhaps prematurely, an opinion different from that of the committee. With the best motives and purest intentions, well informed men are not always sufficiently guarded against erroneous conclusions; and lest we might ourselves incur the like danger, [God forbid,] we will here state *certain principles and characteristics, essentially* appertaining to typhus fever, by which it will afterwards appear easy to define the species of disease now under consideration." Report, p. 13, 14. What an agreeable mixture of affected philosophical precision, of apparent *modesty*, and undisguised self-complacency there is in all this!

Now let us see what are those "essential characteristics," by which the committee are to distinguish with papal infallibility between typhus and yellow fever.



“Medical writers apprise us that the first (typhus) disappears in warm weather, that it is most prevalent during cold weather, more particularly if accompanied by humidity of the atmosphere; that the young and robust are the least liable to its attacks, whereas the old, and those debilitated from want of proper food and comforts of life, are its more general subjects; and that it is unexceptionably a fever of a continued type, of protracted and uncertain stages.” Report, p. 15. We wish the committee had been so condescending as to tell us who the “medical writers” are, that gave them all this wonderful information. As they have not done this, we shall adduce such authorities as we have at hand, to show that all these “essential” and infallible characteristics are either wholly incorrect, or inapplicable to the purpose for which they are advanced.

The *first* diagnostic which they give us is, that typhus fever *never prevails in warm weather*. A good deal of stress is laid upon this point in various parts of the Report, and in one place it is even asserted to be “repugnant to reason and common sense,” to call the Bancker street fever typhus, because typhus is “*a fever generated in a cold, and extinguished by a hot temperature.*” Report, p. 29. Now let us compare this with the statements of Pringle, Willan, Bateman, &c. &c. on this subject. Sir John Pringle, in his observations on the hospital or jail fever, which is the same as the typhus gravior, tells us that “the hospitals of an army, when crowded with sick, or when the distempers are of a putrid nature, or at any time when the air is confined, *especially in hot and dry weather*, produce a fever of a malignant kind, and very mortal.”\* In confirmation of this, he states, that in 1743 the hospital or jail fever prevailed in the army in the month of July and August.† In 1746 it broke out in May and June,‡ and in 1748 it raged in July, when the weather is described as having been extremely hot and dry.§

Dr. Willan describes typhus fever as occurring in London in August and September, and adds, “this disease is extended by

\* Observations on Diseases of the Army, &c. p. 291.

† Ibid. p. 27.

‡ Ibid. p. 57.

§ Ibid. p. 75.

infection, during the months of October and November, but its progress is generally stopped by the frosts of December.”\*

Dr. Bateman says, “at all times there appears to be a greater disposition to fever in London during the autumnal months, which diminishes with the approach of winter. The present epidemic, (typhus,) if we may deduce such an inference from the demands upon the house of recovery, was most particularly prevalent from the beginning of August to the middle of November. The monthly admissions were—in June 28, July 22, August 67, September 81, October 109, November 92, December 68.”†

In 1817 and 18, typhus raged epidemically in Belfast, Ireland. It commenced in May, and prevailed extensively throughout all the summer and autumnal months.‡ We shall now quote two or three American authorities, to prove the same point. A very sensible physician and writer, Dr. Gallup of Vermont, says, “this disease (typhus) prevails most frequently in August and September; but no month is free from it, especially the fore part of the cold season.”§

The 7th volume of the New England Journal of Medicine and Surgery, contains an account of a typhus fever, which prevailed in the Boston alms-house in 1817, 18, and which broke out in the month of May.|| And the very last number of the Medical Recorder gives a description of a typhoid fever which was prevalent in Philadelphia during the last summer and autumn.

We might go on and bring forward authorities without end on this head, but we shall rest content with a single additional one, which approaches so nearly to the nature of an *argumentum ad hominem*, that we cannot deny our readers the satisfaction of being put in possession of it. “The principal physician in attendance on the hospitals at Bellevue, (New-York) affirms that, during the

\* Willan on the Diseases of London, p. 43.

† Bateman on the Epidemic, &c. prevailing in London, 1818, p. 16.

‡ See Observations, &c. on the Epidemic in Ireland, by Francis Rogan, M. D. &c. p. 150-1.

§ Sketches of Epidemic Diseases, in the state of Vermont, &c. by Joseph A. Gallup, M. D. p. 363.

|| See vol. 7. p. 105.



months of July, August, and September, and the early part of October, there occurred at that establishment, a disease exhibiting many characteristics of the *typhus gravior*, jail, or hospital fever, &c." Report, &c. p. 11. Will it be credited that this is a literal extract from the same Report which pronounces it "repugnant to reason and common sense" to suppose that typhus fever can prevail in warm weather? and yet it is even so. That the committee may believe and assert things "repugnant to reason and common sense" we readily admit, because we think, that they themselves have satisfactorily made out the case; but that the other authorities whom we quoted should be involved in the same censure, we must protest against most unequivocally. We believe no one will pretend to deny that Pringle, Willan, Bateman, &c. are very competent judges on the subject we are discussing; and if so, the conclusion is inevitable that the framers of this Report have been convicted of gross ignorance, or wilful misrepresentation. As we wish to be accommodating, we will leave it entirely to the committee to make their own election.\*

The *second* diagnostic of typhus and yellow fever which is given in the Report is, "that the young and robust are the least

\* Since the above was written, we have seen the second No. of the "Philadelphia Journal of the Medical and Physical Sciences," containing a paper on yellow fever, by Dr. Jackson, President of the Philadelphia Board of Health. We regret exceedingly that a gentleman of Dr. Jackson's standing, should have so far forgotten the respect due to himself and the public, as to have written in a style so intemperate and unwarrantable. With Dr. J.'s personalities, however, we shall have nothing to do. But to give the reader a specimen of his medical knowledge, we shall quote the following swaggering paragraph. "*All* experience and *all* authority establish the reverse position as true, and typhus is admitted by the most eminent and skilful of the profession to be abated and destroyed by the heats of summer, and to flourish and be ripe in the colds and damps of winter." p. 538. We do not know who Dr. Jackson would consider "the most eminent and skilful of the profession," but he certainly would not deny that Pringle, Willan, Bateman, &c. ought to be ranked among that number. If so, we wish him to compare his assertion with the quotations we have given from the works of these writers.

Besides, Dr. Jackson, in this very paper, describes "a fever of a bilious and remittent character, combined with *typhoid symptoms*," which prevailed among the blacks in Philadelphia, in the months of *May, June, July, and August*. See p. 321. And the editor of the Journal, in speaking of the Bancker

liable to the attacks of typhus, whereas the old, and those debilitated from want of proper food and comforts of life, are its more general subjects." Report, p. 15. Against this we have nothing to object, and we congratulate the committee upon having for once stumbled upon something like the truth. But this, so far from supporting what the committee wish to establish by it, must prove fatal to their cause. They would have us believe that the subjects of the Bancker street fever were all of "robust and plethoric habits," and therefore, that fever could not have been of a typhoid character. It is, however, wholly incredible that such could have been the fact. We are told that they lived in confined and filthy apartments, "the haunts of drunkenness and debauchery," &c. Report, p. 6. Under such circumstances, it is not very probable that in the heat of summer, their constitutions would be very robust. What must, however, put this matter completely at rest is, the testimony furnished in the dignified, candid and impartial statement of facts published by the Board of Health in reply to the Report under review. The board state, that "the disease has been almost exclusively confined to coloured persons, and particularly to that description of blacks who lived in crowded apartments, especially in cellars, and who were depraved in their habits and indigent in their circumstances; inasmuch as the black population of that district is almost exclusively composed of persons of that description." Statement of Facts, &c. p. 4. And again, "the disease prevailed almost without a single exception, among those persons who were excessively intemperate, in extreme poverty, and who were crowded together in filthy and confined apartments; especially in low, damp, and ill ventilated cellars: a combination of circumstances, which, it is universally

street fever, says, "we deem it not improbable, as is alleged by Dr. Hosack, that it was of a *typhoid character*, very distinct from our summer pestilence. To this conclusion we are led, independently of our respect for his authority, by the recollection, that at the same time a *similar disease existed to a very considerable extent in our own city. That such was the case, no medical man among us denies.*" Phil. Journal of Med. and Phy. Sciences, No. 2, p. 444. With all this testimony staring him in the face, how can Dr. Jackson hazard the assertion, that the most "eminent of the profession" admit that typhus never prevails in warm weather?



admitted, produces fever of a putrid or typhoid character, whereas yellow fever makes no distinction of persons, and is equally fatal to the rich and to the poor." Statement of Facts, &c. p. 7.

Our own personal knowledge goes to confirm in every respect, this account given by the Board of Health, which we think overthrows completely the assertions of the Report, and establishes, beyond any dispute, the fact, that the inhabitants of Bancker street were the fit and appropriate subjects for fever of a typhoid character.

The *third* "essential" and infallible "characteristic" of typhus, as we are told in the Report is, "that it is *unexceptionably* a fever of a continued type, of protracted and uncertain stages." Report, p. 15.

With all due deference to the infallibility of the committee, we must beg leave to contradict, in unequivocal language, the assertion, that typhus is "*unexceptionably a protracted fever.*" Sir John Pringle, in speaking of hospital fever, says that "when the air is at the highest pitch of malignity, the course of the disease comes to be *very rapid*, so as to terminate in *five or six days*, in death or a favourable crisis."\* Wilson says, "typhus sometimes terminates in 10 or 12 days, *or within that period.*"† Both Drs. Bateman and Rogan, in the works already quoted, relate cases of typhus terminating on the 6th and 8th day of the disease,‡ and Dr. Armstrong in his invaluable Treatise on Typhus, describes cases in which it proved fatal in 48 hours.§ Even Dr. Thomas, an author from whom we suspect most of the medical knowledge contained in the Report has been derived, speaks in the following terms: "In warm climates it (typhus) seldom continues above *a week or ten days, if so long.*"||

We have thus gone through the examination of these "essential characteristics" of typhus fever, by which the committee were to

\* Observations on the Diseases of the Army, &c. p. 304.

† A Treatise on Febrile Diseases, by A. P. Wilson Philip, vol. i. p. 137. Am. Ed.

‡ Bateman on Contagious Fever, &c. p. 50. Rogan on Epidemic of Ireland, p. 25.

§ American edition, p. 69.

|| Practice of Physic, p. 52. 4th Amer. Ed.

decide at once concerning the character of the Bancker street fever; and we have been thus minute, not merely because we wished to expose the crude notions of the committee, but because this is really the basis of all their subsequent deductions and assertions. For example, they reason thus:—If we can only make the public believe by a bold assertion, that typhus fever does never prevail in warm weather, then we are safe in our inference that the Bancker street fever was not typhus. Such we say has been the manner in which the committee have reasoned, and we think we have successfully attacked the foundations upon which the whole of their superstructure has been reared.

Having laid down their “essential and infallible characteristics” of typhus fever, the committee next approach yellow fever; and here we have another fine specimen of medical erudition and philosophical acuteness!

“A very different and quite opposite series of *essential and characteristic* circumstances, denotes our ordinary bilious, malignant, remittent fevers, or yellow fever; even supposing for the sake of argument, that this should be considered a disease *sui generis*.” Report, &c. p. 16. From the language here used, it is evident that the committee intend to convey the idea that ordinary bilious fever, malignant fever, remittent fever, and yellow fever are all the same disease. This is really out-heroding Herod; and we suspect the committee will find very few, even of their own party, to subscribe to this doctrine. Even Dr. Rush, who with so much ingenuity has maintained the celebrated sophism of the unity of disease, never went quite as far as this. He believed indeed that yellow fever belonged to the same type of fever with the ordinary bilious remittent, but he admitted that the former was of a higher and fiercer grade, and distinguished by a train of symptoms not to be detected in the latter. For a practical proof of this we refer to Dr. Rush’s description of the remitting fever which occurred in Philadelphia in 1780,\* the symptoms and treatment of which will be found very different from those of yellow fever. We shall now very briefly look at those “*essential characteris-*

\* Medical Observations and Inquiries, vol. 2, p. 385. 3d edition.



*tics*" of yellow fever given us in the Report, and by which it may be so infallibly distinguished from typhus. "First. It has a remittent, or an intermittent type." Report, p. 16. We are aware that on this subject distinguished physicians hold very opposite opinions. But as far as we have been able to make up an opinion, it is decidedly in favour of its being a *continued*, and not a *remittent*, and much less an *intermittent* fever. We shall strengthen this opinion by one or two authorities, and shall purposely quote non-contagionists, against whom of course the committee will have no objection. Dr. Mosely has written very ably and learnedly to prove the identity of the yellow fever and the *causus* of the ancients, an inflammatory fever of continued type; and he says, "those who have mistaken bilious remittent for this fever, consequently speak of *remissions* which do *not* happen in this disease."\*

Dr. Revere, in his account of the yellow fever of Baltimore in 1819, asserts positively, that "there were no distinct remissions or exacerbations of fever. After the first attack there was generally an *uninterrupted febrile state*, until a complete intermission took place." This is part of a description of yellow fever by Dr. Revere, which is justly quoted by Dr. Watts (*a member of the committee*) in his paper on yellow fever, as a most "excellent description" of the disease.†

These quotations are sufficient to show that accurate observers, even among the non-contagionists, do not support the doctrines advanced by the committee.

But even admitting for the sake of a little argument, that the yellow fever is of a remittent type, we assert that this is by no means an infallible diagnostic between it and typhus. In proof of which take the following from Pringle:—Hospital fever, "though of the continued kind, has often exacerbations at night, with remissions and partial sweats next day; and after a long continuance, is apt to change into a hectic, a remitting or intermitting form."‡

\* Mosely on Tropical Climates, &c. p. 143.

† See Hospital Medical and Surgical Register, p. 146.

‡ On Diseases of the Army, &c. p. 308.

The committee tell us that, "Secondly, the progress of yellow fever is rapid and violent." Report, p. 16. This we admit, but we have also shown that typhus gravior is frequently equally rapid in its course. This, therefore, can avail nothing as a diagnostic between these two diseases. "Thirdly, Yellow fever is expected never to break out, excepting during, or immediately following the warmest season of the year: and frost always stops its progress, and puts an end to its existence as an epidemic." Report, p. 16. This is equally inconclusive with the preceding as a diagnostic; because we have proved that typhus fever often prevails in the warmest season of the year. Besides, it is well known to every physician in New-York, that cases of the Bancker street fever continued to occur even after the ground was covered with frost and snow. See "Statement of Facts," &c. p. 9. So that, from the very data furnished by the committee themselves, the conclusion is inevitable, that the disease of Bancker street could not have been yellow fever.

"Fourthly. Yellow fever attacks the most robust, seldom the infirm, or those in the decaying stages of life." Report, p. 16. This is true—but it proves just the reverse of what the committee wish, for the victims of the Bancker street fever, so far from being robust, were worn out by debauchery and intemperance.

"Fifthly. Yellow fever shows three stages, one inflammatory, of 36 hours; one asthenic, and one a series of deadly symptoms, the forerunners of certain death." Report, p. 16, 7. How does this compare with the first diagnostic, in which we are told that yellow fever is of a *remittent* or *intermittent* type. There seems to be rather an awkward contradiction here.

As the last distinguishing characteristic of yellow fever, we are told that it "may at any period exhibit peculiar bilious or yellow effusions, in the eyes, skin or parenchyma of the liver, and in the stomach, the formation of dark brown or black matter; hence black vomit." Report, p. 17. That bilious effusions are by no means pathognomonic of yellow fever, every tyro in medicine knows, and that they may and do occur even in typhus is admitted by the committee in this very Report, when they tell us that in *July, August and September, typhus gravior prevailed in the*



*Hospitals at Bellevue, "accompanied with bilious symptoms, such as yellowness of the eyes and skin, &c."\**

That black vomit may also occur occasionally in typhus, we shall satisfactorily prove directly. Such are the *essential characteristics* of yellow fever as furnished by the learned committee of the Medical Society, which, like the "*essential characteristics*" of typhus are shown to be either totally erroneous, or proving most conclusively that the fever of Bancker street was not yellow fever.

We might now bring our remarks to a close, being satisfied that every man of sense and candor must have been long ago convinced of the egregious error into which the committee have fallen, in attempting to prove that the Bancker street disease was yellow fever. But that the committee may not have it in their power to complain that we have not given them a thorough sifting, we shall go on a little further, and notice the description which they give of the fever. And here we must candidly say, that we do not recollect to have met with an account of any disease more crude, incongruous and unprofessional; and we think we can find a very satisfactory cause of this, in the manner in which we suspect the Report has been compiled. As some of the committee had not seen a single case of the Bancker street fever, and others but very little of it, communications were obtained from a number of physicians who had attended cases of it, and from these documents the report was drawn up. Under such circumstances it is natural to suppose, that it would be strongly characterized by contradictions. It is also easy to conceive, that from such materials, it might with equal ease have been proved that the fever of Bancker street was the plague, or almost any disease in the nosological catalogue. For example, one physician writes to the committee that a patient of his had a *red and inflamed eye*—a second writes that he had a patient with a *yellow skin*—a third writes that he had a patient with *great præcordial distress*—a fourth, that he had a patient with *black vomit*—a fifth, that he had a patient covered with *petechiæ*—a sixth, that he had a patient with

\* Report, &c. p. 11.

*hæmorrhage*, &c. and so on. Now here are many of the characteristic symptoms of yellow fever. Put them all together in a general description, and you make out a case of that fever. Whereas in reality, so far from any thing like yellow fever, the first patient may have had nothing more than ophthalmia—the second, jaundice—the third, pneumonia—the fourth, gastritis; and the fifth and sixth have been in the last stages of typhus gravior. We hope we shall not be misunderstood: we do not say that the committee have drawn up their report in this way. But we should be much obliged to them to explain their meaning, when they say, that “in all cases *every symptom* did not occur, but *in all*, some of the distinguishing symptoms did exist.” Report, p. 20.

We shall now proceed to offer a few remarks, upon some of the more prominent and decisive symptoms of the Bancker street fever, as detailed by the committee, *taking it for granted*, that their account is correct. And 1st, of the *black vomit*. There is not a little art displayed in the manner in which the committee introduce this symptom. They say, “Hiccough often attended the last stage for twenty-four hours preceding death; and a vomiting of dark bilious matter, frequently of a brownish black colour. From the total want of conveniences in the habitations of the sick, it was but seldom practicable to procure a sight of the matter ejected or dejected; the whole of their bed-room furniture seldom supplying more than one vessel for the use of the sick, and that one appropriated at different times to various uses, as necessity might demand. The matter of black vomit has been witnessed by many of your committee.” Report, p. 23–4. It is very evident, we think, from this, that very few cases of black vomit have occurred in this disease, and yet the language here used is so general, as to convey the idea that this was a symptom occurring pretty generally in all the cases of fever. That this is not true, is established by testimony in the possession of every physician in New-York. Out of nearly one hundred cases of this fever at the Bellevue Hospital, not a single instance of black vomit is pretended to have occurred. The same may be said of upwards of fifty cases of the same disease at the New-York Hospital. From



the statements contained in the Report, it appears however, that the committee have met with *eight* cases of what they call black vomit, out of upwards of two hundred cases of the fever. This is the extent of what they pretend, and it is sufficient to prove that the fever could not have been yellow fever. Admitting that this was genuine black vomit, (of which there is no little doubt) we would ask, is this *pathognomonic* of yellow fever? That it is not, has been very satisfactorily shown by Dr. Edward Miller, in his remarks on yellow fever.\* But more than this, we can prove that black vomit has occurred occasionally in typhus fever, and therefore it is not at all strange that it should have been met with in a few cases of the typhus gravior in Bancker street. Dr. Bard of New-York, one of the most respectable physicians of our country, in a letter on yellow fever, says "now and then, and particularly during the war, when the city (of New-York) was much crowded, and little attention was paid to cleanliness, fevers of a more malignant nature have prevailed, in which a foul mouth, hæmorrhages, petechial eruptions, and other marks of dissolution have either characterised the disease from its commencement, or been superadded to the bilious symptoms in the latter stages; and then the disease has been termed malignant, putrid, petechial, *jail or hospital* fever. In such cases I have now and then seen profuse bloody discharges, and *black*, or as it is now more generally named, *coffee ground vomiting*."† He then goes on to show that this fever was very different from yellow fever. Dr. Gallup also speaks of cases of typhus in which the patients "had the yellow tinge and black vomit."‡ And the same writer mentions two cases of the black vomit occurring in the spotted fever.§

2d. The *yellow suffusion of the eyes and skin*, is another symptom dwelt upon to prove that this was yellow fever. We have already shown very conclusively, that this is by no means pathognomonic of yellow fever. Every writer on typhus gravior tells us that bilious symptoms not unfrequently appear in that

\* Medical Repository, vol. ii. p. 379.

† Additional Facts and Observations by the College of Physicians, Philadelphia, &c. American Medical and Philosophical Register, vol. i. p. 480.

‡ Gallup on Epidemics, p. 358.

§ Ibid. p. 231.

disease, especially when it prevails in warm weather; and the committee themselves admit this, when they tell us that the *typhus gravior* in the Bellevue Hospital, was attended “with *yellowness of eyes and skin*” and other bilious symptoms. Report, p. 11. We may just remark here also, that the yellowness observed in the fever of Bancker street, was very different from the *peculiar dingy colour* of yellow fever, as we ourselves have remarked. Besides, what throws still more light upon this point is, that as the season advanced and the cold weather set in, the fever was disrobed in a great measure of these bilious symptoms; showing conclusively, that they were not essential to the disease, but were occasioned by the heat of the weather.

3d. The next symptom we shall notice, is the state of the bowels: “Nausea and vomiting of green bilious matter *usually* took place at the onset of the disease, with *frequent stools* of green copperas coloured water.” Report, p. 21. Until this time we confess we never knew that a diarrhœa was one of the first symptoms of yellow fever; and we profess ourselves wholly indebted to the committee for this wonderful piece of information. Heretofore we believed what is stated by Lining, Rush, and every other writer on yellow fever, that the bowels were obstinately costive in that disease, and most especially so at the commencement of the fever. We think too, that we can prove that a diarrhœa, so far from being a symptom of yellow fever, is frequently a symptom of typhus. Pringle says, of the hospital fever, “when the patients lie cold, as they often do in field hospitals, the pores of the skin being shut, a *diarrhœa* is a common symptom, but then is never critical.”\* So too, says Dr. Bateman:—“Occasionally a *diarrhœa* occurred among the earliest symptoms of the fever.”†

4th. Soreness and tenderness on pressure of the epigastric region, is another symptom adduced by the committee. That this *did* occur generally in the typhus gravior at Bellevue, the committee themselves admit. See Report, p. 11.

\* Observations on Diseases of Army, p. 297.

† On Contagious Fever, &c. p. 57.



5th. *Petechiæ*, hæmorrhages, &c. are common symptoms in typhus gravior. The committee may learn this by reading any book which treats of that fever.

6th. The duration of this disease. On this point we must unequivocally charge the committee with unfairness in their statement. They uniformly hold out the idea, that the disease terminated in five or six days. This we pronounce absolutely untrue, and the committee must know it to be so. We have before us, a document from a physician who saw nearly one hundred cases of the fever, in which he mentions, that in many cases, death did not ensue until after an extended period of two, three, and even four weeks; a fact totally irreconcilable with the supposition of its being yellow fever. That some instances occurred of death on the fifth or sixth day of the disease, we do not deny; but this we have already shown, frequently occurs in typhus gravior.

We have thus examined the prominent symptoms of the Bancker street disease, and the conclusion is inevitable, that it was unquestionably a fever of a typhoid character, and that the committee of the Medical Society of New-York, have committed a most gross error in calling it yellow fever.

Concerning the true nature of the Bancker street fever, the board of health have published so cogent and unexceptionable a statement, that we cannot deny ourselves the pleasure of giving it to our readers.

“From the foregoing statement of facts, and the proofs which are subjoined, and from our own personal observations, we are irresistibly led to the conclusion:—

That this disease was generated by human effluvia, confirming the opinion heretofore given, that its predominant character was typhus; assuming, in some instances, the form of typhus gravior, jail or ship fever; and in other instances, attended especially by bilious symptoms, particularly during the extreme and continued heat of the weather.

1. Because the disease prevailed almost without a single exception, among those persons who were excessively intemperate, in extreme poverty; and who were crowded together in filthy and confined apartments; especially in low, damp, and ill-ventilated

cellars: a combination of circumstances which, it is universally admitted, produces fever of a putrid or typhoid character. Which is furthermore confirmed by the fact, that where this combination of circumstances existed in other parts of the city, the disease made its appearance there also. Whereas yellow fever makes no distinction of persons, and is equally fatal to the rich and to the poor.

2. Because, those persons who were comfortably accommodated, who were regular in their lives, and did not enter into the infected apartments or dwellings, escaped the disease; which is diametrically opposed to the known laws of epidemical diseases: and it is well known to every inhabitant of this place, that no such exceptions were observed in those portions of our city where yellow fever has prevailed.

3. Because, on the removal of the sick from their contaminated apartments to the New-York and Bellevue Hospitals, where they were placed in clean and airy wards, the disease became, in conformity to the laws of typhus, milder in its character, and more manageable in its treatment. Whereas, in yellow fever, no evident abatement of the symptoms have heretofore been observed in persons sick with that disease, when removed to the public institutions, established for their reception, in the vicinity of this city. But, on the contrary, the records of such establishments show as great, and in some instances a greater, mortality, in proportion to the number of sick, in such places, when compared with those who remained in the city. And when we compare the deaths to the number of sick, of the cases occurring in Bancker street after being removed to the New-York and Bellevue Hospitals, with those which took place from yellow fever in such institutions in 1803, 5, and 19, we find that in the latter disease nearly one-half died. Whereas, of this disease, the proportion of deaths to the sick in the infected district was as one to three; in the hospitals at the alms-house as one to five; and in the New-York Hospital as one to six.

4. Because, notwithstanding the white population was three times the number of coloured persons in the district where this disease occurred, yet was the disease almost entirely confined to



blacks. While, on the contrary, in yellow fever blacks have always been considered to be less susceptible of the infection than whites, insomuch that coloured persons have, for this very reason, been employed in nursing the sick, and in the interment of the dead.

5. Because, out of 237 cases, the total number which occurred of this disease, no symptom presented itself different from those which usually attend typhus fever under similar circumstances. As it is not even pretended, that out of all this number more than eight cases have occurred of black vomit, some of which are stated to have been ascertained *after the death* of the patient!\* Whereas, in yellow fever, at least one-half of those who die have this characteristic symptom.

6. Because, a high grade of typhus, attended with bilious symptoms, has oftentimes occurred in this as well as in other countries, at the same season of the year at which this disease prevailed. And a very remarkable instance of it took place in the year 1801, when many vessels, unusually crowded with passengers, arrived at this port from Ireland during the summer and autumn. Out of at least 750 who were admitted into the Marine Hospital, almost all of whom were sick of this disease, nearly 300 died, besides a great number who perished on their passage. The disease, which was simply typhus, on board the ships which first arrived became, as the vessels progressively arrived later and later in the season, (from the same ports,†) combined with bilious symptoms, which acquired more and more intensity as the season advanced.

7. Because, the disease has continued to prevail in the same district, and among the same classes of people, notwithstanding the prevalence of constant cold and of repeated frosts and snows; cases having been received into the New-York Hospital as late as the last day of December, 1820, and attended with the same

\* See Pamphlet of the Medical Society, pp. 12, 14, and 26. This is on the supposition, that the aforesaid black vomit consisted of the flaky or coffee-ground matter, which characterises yellow fever; but of this there is no evidence given.

† Chiefly from Belfast.

symptoms and circumstances. Whereas, in the yellow fever, it established beyond a doubt, that frost invariably and effectually extinguishes the disease.

8. Because, cases of this disease have not only been brought to the New-York Hospital since the commencement of freezing weather, but when the severity of the cold would not admit of free ventilation in the wards of the house, the disease has, in conformity to the laws of typhus, been in many instances communicated to the nurses and patients of that institution;—as appears by the Report of the governors to the board of health.” *Statement of Facts, &c.* pp. 6, 7, 8, 9, 10.

All this is supported by a large body of notes and proofs, making together a mass of doctrine and of testimony which is perfectly irresistible, and dissipates into air all the crudities and misstatements of the Report of the committee of the Medical Society.

In addition to the foregoing reasons to prove that the Bancker street disease was not yellow fever, we will just state that in the treatment of it, *emetics* have been found uniformly of the most unequivocal benefit: a class of remedies which every practitioner knows to be fatal in yellow fever. This fact of itself, independent of every other consideration, would be sufficient to show the egregious error into which the committee have fallen, in designating the character of this disease. And further, the convalescence has been observed to be generally very tedious, corresponding precisely with the character of typhus; whereas in yellow fever it is known, that the convalescence is astonishingly rapid.

We have now gone through so much of the Report as relates to the nature, causes, &c. of the Bancker street fever: and have placed, we think, in sufficient relief, the ignorance, absurdities, contradictions, and misstatements with which it abounds.

The remainder of the Report relates to the New-York Board of Health, and deals in the most unjust and gratuitous censure of that respectable body, for not believing that the disease of Bancker street was yellow fever. We had designed originally, to enter somewhat minutely into the charges brought against the Board in this Report; but we have already far exceeded the limits



prescribed to these remarks, and on reflection, we think it impossible that the Board can at all be injured by the attacks of such bungling assailants. We shall, therefore, merely express in general terms, our approbation of the conduct of the Board of Health. They have signalized themselves by the intelligence, promptitude, and firmness, with which they discharged the sacred trust of guarding the lives and health of their fellow citizens. And during no season have they been better entitled to the gratitude of the public than that of the last year, when but for their prudence and firmness, the false and malicious rumours so industriously circulated that the yellow fever was raging in New-York, might have filled that city with distress and confusion. On this point we shall add no more, feeling satisfied that it is unnecessary.

We intended also to have given some specimens of the style in which this *wonderful* Report is written, but we have no room for them. We will just state, that we do not recollect to have ever met with a better illustration of what Horace describes in the beginning of his Art of Poetry, than the whole texture and style of this Report.

Humano capiti cervicem pictor equinam  
Jungere si velit, et varias inducere plumas,  
Undique collatis membris, ut turpiter atrum  
Desinat in piscem mulier formosa superne,  
Spectatum admissi risum teneatis, amici?  
Credite, Pisones, isti tabulæ fore librum  
Persimilem, cujus velut ægri somnia, vanæ  
Fingentur species; ut nec pes, nec caput uni  
Reddatur formæ. Hor. Ars. Poet.

As we quote this for the benefit of the committee, we shall give them a translation of it.

Suppose a painter to a human head  
Should join a horse's neck, and wildly spread  
The various plumage of the feather'd kind  
O'er limbs of different beasts, absurdly join'd;  
Or if he gave to view a beauteous maid  
Above the waist with every charm array'd,  
Should a foul fish her lower parts enfold  
Would you not laugh such pictures to behold?  
*Such is this book, that like a sick man's dreams,  
Varies all shapes, and mixes all extremes.*

Francis' Trans. of Hor.

In contemplating this Report, we have been struck with a singular coincidence, which may in some measure account for the strange incongruities that are heaped together. The committee is composed of *seven* members. Now it is well known that *seven* has, from the earliest ages, been considered a mystical number. It was so among the Jews, and among the Greeks; we read of the *seven* wonders, and the *seven* wise men. Whether the Medical Society thought of this when they appointed a committee of *seven*, we do not know, but we think it more than probable. At any rate, we think the notion of the ancients on this subject is quite applicable to the committee. The number *seven* is mystical—the *seven* committee men are mystical—and the whole Report is mystical.

We shall now take leave of the committee, and as we wish to part upon good terms with them, we will give them a piece of advice, which if they duly improve, may be of infinite service to them hereafter, and the suggesting of which we hope will entitle us to somewhat of their gratitude.

Sumite materiam vestris, qui scribitis, æquam  
Viribus; et versate diu quid ferre recusent,  
Quid valeant humeri. *Hor. Ars. Poet.* 38. 40.

B.

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ART. XIII. *Facts and observations on Liver Complaints, and bilious disorders in general; and on those derangements of that important organ, and of its immediate or intimate connections, which most sensibly influence the Biliary Secretion, with practical deductions, drawn from an ample share of experience, and a close and constant attention to this subject, in various climates; connected by an appropriate and successful mode of Treatment. The whole illustrated and confirmed by an extensive selection of Cases, &c. &c.* By JOHN FAITHHORN, M. D. formerly Surgeon in the honourable East India Company's service; 8vo. pp. 158. First American edition, Hickman and Hazzard, Philadelphia, 1820.

THE extensive prevalence and increasing frequency of hepatic disorders, render interesting every attempt to elucidate the patho-



logy of the liver, and to improve the treatment of its diseases. The work of Dr. Faithhorn, however, does not derive its interest from this circumstance alone. The reader does not proceed far in its perusal, before he discovers the author to be possessed of an extensive and minute acquaintance with his subject; to be a man of large and various experience in the treatment of hepatic affections; and to possess the happy facility of communicating his observations in a style at once chaste, perspicuous and enlivened. But this is not the place for criticism. We proceed to present our readers an analysis of the work. After some preliminary observations on the great importance of the liver in the animal system, the author proceeds to give a general view of its structure and functions. He considers its peculiar organization, its natural colour, its usual weight, its situation, its anatomical connections and relations, its principal parts, &c. Its principal parts he successively considers under the several heads of ligaments, surfaces, margins or edges, tubercles, lobes, and vessels. He considers the liver as a grand reservoir to receive the returning blood from the various parts of the body, charged with the several impurities it has imbibed in its progress, and as a glandular apparatus for the conversion into bile of the collected colluvies of the constitution.

He next proceeds to consider the secretion of the bile, its uses, its chemical analysis, &c. Its principal use, according to our author, is to separate and discharge the feculent part of the aliment. Yet he believes that it is intended to serve other purposes in the economy, as so large and complicated an organ is destined for its preparation. He supposes it to be necessary for the perfect assimilation of the aliment; and that it imparts to the chyle some peculiar principle, which conveys renewed energy to the general mass of fluids. He minutely describes and considers the colour, consistence, smell, taste, and healthy quantity of the bile; and then proceeds more particularly to notice its different offices. He views this secretion as intended:—1. To extricate the chyle from the chyme. 2. To excite the peristaltic action of the intestinal canal. 3. To impart colour to the fæces. 4. To prevent the accumulation of mucus by its abstergent and attenuating proper-

ties, and to neutralize any disengaged acid in the *primæ viæ*. 5. To impart and uphold the wonted vigour of the lymphatic system.

After this general view of the structure, functions and economy of the liver, he proceeds to the consideration of its diseases. He makes some general remarks upon bilious disorders, and upon the influence exerted over them by the conditions of the spleen and pancreas; and introduces the subject of *acute hepatitis*. This disease he describes with admirable precision and particularity, entering pretty fully into the *ratio symptomatum*. In the treatment of this complaint he does not advance any thing new; but is very strenuous in recommending the antiphlogistic practice in its fullest extent. Large and repeated blood-letting he strenuously advocates, and advises, with great confidence, the application of cups, or leeches, over the region of the liver.

He next takes up the consideration of *chronic hepatitis*. This he considers as very generally prevalent, in an insidious form, even in Europe. Indeed, he considers the liver as the chief seat of most ailments *unattended by febrile action*; ascribing to some derangement of the organ, every chronic or lingering illness. He enumerates the symptoms of this form of disease very minutely and clearly. He notices as an occasional attendant of this disorder, a peculiar sensation of fulness in the throat, as if an extraneous body were lodged there, accompanied with a transient feeling of distress in the adjacent parts. The chronic hepatitis, as described by Dr. F. exhibits much protean variety in the extent and number, as well as degree of its symptoms. Yet he does not believe it to be so difficult of detection, but that it may be ascertained very readily by the *touch*. The examination should be conducted with some nicety. Perhaps we could not do better than transcribe his directions upon this subject. "In examining the hepatic region by pressure," says Dr. F. "we should take advantage of that position in which the muscles of these parts are most relaxed: if the patient be placed horizontally, as in bed or on a sofa, then the legs should be drawn up, or if in the upright posture, in that case he should rest his hands on a table, and lean with his body bent forwards. But in some cases the enlargement



of the liver, with the descent of its margin, is to be more satisfactorily ascertained by grasping the integuments of the belly as if you expected to lift up the acute edge of the organ, than merely by pressing with the point of the finger." p. 35.

Dr. F. considers the intercommunion of all the abdominal viscera with the liver, as so intimate, that they are almost necessarily involved in an affection of this organ, and that it, for the same reason, partakes of their derangements. Some difficulty occasionally occurs from this circumstance, in determining whether the liver is primarily affected, or whether it is merely sympathising with some remote abdominal irritation. He has observed too, that patients labouring under chronic hepatitis are remarkably susceptible on the slightest exposure to sudden colds about the head, throat and chest. This accident will sometimes obscure the true nature of the complaint, and cause it to be mistaken for bronchial and pulmonic affections. In order, therefore, to arrive at the proper method of detecting it, and to ascertain its most effectual mode of cure, he proceeds to consider chronic hepatitis in all its various deceptive modifications, and in all its complicated varieties.

1. He views the disease as connected with, or counterfeiting *pulmonary diseases*. He shews that a troublesome cough will sometimes attend an hepatic disorder, so as to mislead the physician into the belief that his patient labours under extensive disease of the lungs, when these organs are perfectly sound. He shows too, that disease of the pulmonary organs, simulating chronic catarrh, asthma, and even phthisis, may be induced by sympathy with a diseased liver, or by the mechanical irritation they receive from morbid enlargements of this viscus. He hence deduces the great necessity of nice examination in all apparently pulmonary disorders, as their treatment must essentially differ according as they may be primary, or sympathetic. We would recommend this chapter, and the cases illustrating the principles it contains, (p. 107–113.) to the attentive study of American physicians in particular. Pulmonary diseases are exceedingly prevalent among us. Would not their fatality be diminished by more attention to their causes?

2. He next considers chronic hepatitis as complicated with diseased *mesenteric glands*, and the serious and alarming sequelæ of this distressing complaint.

3. The influence which a morbid condition of the liver has in inducing disease of the *stomach*, forms the subject of the next section. The author here shows how essential a healthy state of the liver is to the due performance of the gastric functions; and how certainly a vitiation of the gastric juice, the generation of acid in the *primæ viæ*, imperfect chylication, flatulence, and all the other morbid symptoms which prove so distressing to the dyspeptic, are induced by derangements in the hepatic system.

4. *Headache*, as occasioned by sympathy with disease of the liver, or by the irregular distribution of the blood attendant upon enlargements of this organ, forms the subject of this section. The author is not as particular on this subject as we might desire; and what he gives us, only makes us regret that he had not said more.

5. The powerful and extensive influence of the liver on the *nervous system*, and through this upon the *mind*, is next considered. Hysteria, hypochondriasis, melancholy, and many other nervous disorders, are shown to be frequently ascribable to a diseased liver. The author traces *suicide* to the same source; showing that in cases of this nature a fault in the biliary secretion has, upon minute examination, been found quite sufficient to account for the despondent feelings which led to the horrible transaction. We here too have to regret, that the author has been so brief. But perhaps a more extended consideration of the subject would not comport with the unity of the book, or with the strictly practical character which the author seems solicitous to maintain.

6. Latent hepatic obstructions under the guise of *chronic debility*, are next investigated. The author endeavours to show, that from a want of the stimulus of the bile, the absorbents become languid and incapable of acting with sufficient energy to prepare chyle in the quantity necessary to nourish the body, and that the food, in such cases, does not supply adequate nutriment, as it is not suitably assimilated to recruit the defective juices, so as to replenish the continual waste of the constitution. The author



properly condemns the use of tonics and corroborants in such cases, as it is only by restoring the discerning function of the liver that the strength can be regained. In no case is the axiom more true, "*causa dempta tollitur effectus.*"

7. *Flatulence*, as an attendant of liver complaints, Dr. Faithhorn considers no unimportant symptom. He supposes that the true source of this troublesome affection, is too frequently overlooked; that patients and physicians are too much disposed to make light of it, and to refer it to inconsiderable causes, when it is truly indicative of serious derangement in the biliary organs, and may ultimately become the cause of grievous and distressing symptoms.

It would extend our remarks to an undue length, to continue our analysis throughout the succeeding chapters; and as we hope that what we have presented of this valuable work, will induce the generality of our readers to refer to the book itself, we consider it unnecessary to be prolix. Suffice it to say, that the author proceeds to consider the *morbid* and *supersentient sensibility* which so generally attends chronic diseases of the liver. He notices the variations in the *biliary secretion* which are generally observed to attend hepatic affections, and from which useful practical information as to the condition of the viscus, may be derived. And perhaps it is not too much to say, that this is the most valuable section of the work. He points out the morbid changes in the *gastric fluid*, which are frequently the result of liver affections. He considers the diseases of the *intestinal tract* which are connected with, or are the result of biliary derangements. He examines how far the formation of *gall-stones*, *biliary concretions*, and *jaundice*, are connected with a morbid condition of the liver; and, finally, *dropsy*, the common attendant of inveterate hepatitis, is particularly considered.

Having taken this extensive view of the diseases with which affections of the liver may be confounded, and of the complications which they sometimes exhibit, the author enters upon the treatment of chronic hepatitis. He lays it down as an important rule, that, in order to ensure a successful practice, the *excretions must be particularly inspected*, as it is from them only that the state

of the disease can be accurately determined. After some useful practical reflections upon the indications furnished by the discharges, he divides liver complaints into two stages. 1. That of a simple derangement of the hepatic functions; and 2. an actual change in the organization of this viscus. In the *first* case, (the characteristics of which he clearly defines,) he recommends to produce free action of the stomach and bowels; and in cases of congestive plenitude of the organ, to deplete the vessels by abstracting blood from the arm, or by cupping over the region of the liver. The "grand aim," however, he considers, is to "emulge well the biliary vessels." Whenever obstructions of the liver exist, he declares the first indication to be, to clear the bowels of phlegm, and other causes of irritation; and the second, to remove the obstructions of the viscus, to restore the lost tone of the intestines, and to strengthen the system in general. There is nothing particularly novel in the practice pursued by Dr. Faithhorn. He does not think mercury so serviceable, as it generally is in the hepatic affections of warm countries, and rather dissuades from its use in this stage of the disease. He advises, in those cases, where there exists considerable enlargement of the liver and other abdominal viscera, a well adjusted *belt* to be worn around the body. He is very minute in directions for the *diet* of the patient, and the regimen he is to pursue. He ascribes great benefit to *exercise* on horseback in the early stage of the disease, but thinks it too severe when the liver has acquired an enlarged size. *Frictions* with flannel or the flesh-brush, he recommends as a valuable substitute for muscular exertion, in cases where this would be inconvenient or unsuitable. The strictest attention to the warmth and dryness of the feet, he considers indispensable: indeed, a proper regulation of the heat of the body generally by seasonable clothing, he views as a point of incalculable importance. As a valuable auxiliary for the removal of hepatic obstructions, he introduces *warm bathing*. But he thinks that this adjuvant requires very cautious and judicious management. By the assiduous use of these means, Dr. F. declares that the first stage of chronic hepatitis may generally be remedied.

2. From the first stage he proceeds to the more advanced con-



dition of the disorder, where the structure of the organ has already undergone a change from its natural organization. Here, he says, mercury cannot be dispensed with. It is truly a *sine qua non*. In conjunction with this medicine, he administers other deobstruents, and has recourse to local means. As the most valuable of the topical remedies, he recommends the *ammoniac plaster*, (*emplastrum ammoniaci*, Pharmacop. Lond.) prepared with the vinegar of squills instead of the acetic acid, to be applied largely over the region of the diseased viscus. From this application, he says, decided benefit has been derived, even in cases where the liver has been exceedingly enlarged. Although Dr. Faithhorn does not conceal that there are instances in which success cannot be expected, he asserts with confidence, that by an observance of the directions he has given, and due perseverance in the course of treatment he has detailed, *nine-tenths* of the cases that occur may be radically cured. The author concludes his treatise by insisting upon the indispensable necessity of carefully examining the alvine excretions; and by claiming the honour of being the first who has directed the attention of the profession to the important indications which they suggest.

With the view of illustrating the principles and practice detailed in his treatise, the author introduces an extensive and valuable collection of cases at the end of the volume. It is in examining these that we are particularly led to admire his sagacity, and the nicety of his practical skill. D.

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ART. XIV. *Surgical Essays.* By ASTLEY COOPER, Esq. Surgeon to Guy's Hospital, and BENJAMIN TRAVERS, Esq. Surgeon to St. Thomas's Hospital. Part II. second edition, London, 1820.

THE first number of this excellent work has been almost three years before our profession, and the strongest proof of its importance will appear from the fact, that this last part has already gone through two large editions. Indeed had it possessed no intrinsic merits, the celebrity of the gentlemen whose names appear

on the title page, would alone have excited sufficient observation. Perhaps there are not two other individuals in existence, whose association in such an undertaking, could command more general expectations. But the work itself is too interesting, to need the support of distinguished titles; and though by no means adorned with "flowery speech," its sound practical cast cannot fail to engage the attention of sober readers.

With the exception of an interesting paper on iritis in the first part, Mr. Cooper as yet appears the sole contributor. His observations on fractures and dislocations, have occupied not only much of the first, but the greater portion of this volume; and as it is by far the most important as well as interesting article, we notice it particularly. His other communications in the first number, have now been too long before the public to require an analysis.

In the management of dislocations generally, that extraordinary practical sagacity which has so long distinguished Mr. Cooper above all his cotemporaries, is constantly displayed. Among many other ingenious principles, less original however in practice, we were struck with the excellence of one precaution,—to take the muscles by surprise in reduction. It is very natural for the patient, when his attention is directed to the injured part by the efforts of the surgeon, to contract the surrounding muscles; and just in proportion as they are shortened, will be the difficulty of the operation. The irritation of the unnatural situation into which the muscles are thrown immediately after the accident, always produces some contraction; but if this be not augmented by that strong spasm which alarm, and painful extension excite, there will be, comparatively, but little exertion required to effect the reduction. Though this circumstance may have been remarked by experienced practitioners, we do not recollect having observed any allusion to it before the publication of this work; and we cannot avoid noticing it as an ingenious precaution, which every surgeon should recollect. But a few weeks ago, the writer of this article, reduced a humerus which had been seven days dislocated into the axilla, almost instantaneously, while he was asking the patient's permission to make "only an examination." It will always prove a useful method to deceive our patients in



this way, while we are making a sudden effort at reduction, especially if we at the same time, as Mr. Cooper advises, order them to rise from a sitting to an erect posture.

With his characteristic judgment, our author combines in his practice, both the modes of producing muscular relaxation—the bleeding *ad deliquium* of Munro, and the nauseating antimonial plan of Wilmer. In old dislocations, of the femur particularly, it is obvious that the greatest possible relaxation of the muscles will be required, before the extension is made; and we know of no treatment more judicious than the one which Mr. Cooper adopts—to exhibit a nauseating dose of antimonial preparation first, and to follow it with blood-letting *ad deliquium animi*.

Our limits will not permit us to enumerate the ingenious mechanical contrivances, which he recommends for the various dislocations. Every page is filled with interesting practical details, which can only be estimated from an examination of the work itself; while in this hasty analysis it will be impossible to notice more than some of the most novel practices and opinions of Mr. Cooper.

He has most systematically elucidated the differences between all the varieties of fracture and dislocation at the hip joint; and the artificial distinctions of the French authors lose much of their importance when compared with the perspicuity of his diagnostics. The illustrations of his remarks by histories of cases are exceedingly happy; and he has given us in this way not only the results of his own experience, but a most interesting selection from that of his numerous correspondents. As we can designate in the smallest space his diagnoses, we will transcribe a few selected cases.

*Dislocations on the dorsum ilii.* “While walking through the wards of Chester hospital, I examined a case under the care of Mr. Rowlands. *The bone was dislocated upwards, the affected thigh was shorter than its fellow, the knee was inclined inwards and forwards, and the foot was pointed inwards; every attempt to rotate the foot outwards, was productive of considerable pain at the hip.*” “When I had concluded my examination of this case, I was informed that a man had been admitted two months before, under

the care of Mr. Bennett, with a dislocation of the thigh, and having requested of Mr. Bennett the particulars of this accident, he was so kind as to send me the following account.

“John Foster, aged 22 years, was admitted into the Chester Infirmary, July 10, 1818, with a dislocation of the thigh on the dorsum ilii, occasioned by a cart passing over the pelvis. Upon examination, I found the leg shorter than the other, and the knee and foot turned inwards. The patient being firmly confined upon a table, I extended the limb by pullies for fifty minutes without success, and he was returned to bed for three hours; after which, he was put in the warm bath for twenty minutes, and the extension was repeated for fifteen minutes unsuccessfully. I therefore took twenty-four ounces of blood from him, and gave him forty drops of tinct. opii.; continuing the extension, but not succeeding in producing faintness, I gave small doses of a solution of tart. antimonii, which in a quarter of an hour produced nausea; in ten minutes afterwards, I succeeded in reducing the limb, and in less than a fortnight, he left the infirmary quite well.”

“S. R. BENNETT.”

“John Lee, aged 33, of a strong and robust constitution, October 9, 1819, in passing over a foot-bridge, fell from a height of about four feet on a large stone and dislocated his left hip. I did not see him till December 4th, when I found the *limb full three inches shorter, the knee turned in, and the foot directed over the opposite tarsus; the trochanter major brought nearer the spinous process of the ilium*; and on laying the man on his face, the head of the femur and trochanter could be distinctly seen on the dorsum ilii, so as not to leave the slightest doubt of the nature of the injury. With the assistance of a neighbouring practitioner, I immediately set about to reduce it; a girt was applied between the legs, and a bandage above the knee to fix the pullies, &c. in the usual manner. I then made the extension downwards and inwards, crossing the opposite thigh two-thirds downwards; immediately the extension was commenced, I gave him a solution of two grains of tartar emetic, which was repeated five times every ten minutes, but it produced very slight nausea. I shortly after



bled him to sixty ounces without syncope, and after keeping up the extension gradually for about two hours with all the force one man could employ with the pullies, we found the limb as long as the opposite; we then endeavoured to lift the head of the bone over the acetabulum, by means of a towel under the thigh and over one of our heads, at the same time rotating the limb outwards with all the force we were able to exert; the foot at length became somewhat turned out, and the head of the bone to be less distinctly felt, and in about half an hour, we heard a grating of the head of the bone, when the man instantly exclaimed it was replaced, and upon examination finding the foot turned out, the limb of its natural length, and no appearance of the head of the bone on the dorsum ilii, we concluded it must be within the acetabulum, and desisted from any further violence, put the man to bed, and tied his legs together; his foot immediately became sensible, which it had not been before since the accident, and he felt altogether easier. In a few days he could bear slight flexion and extension without pain, and in a week some degree of rotation; the limb became gradually stronger, and the power of motion so increased, that on the twelfth day, he could by himself bring the thigh at right angles with the body; he was taken out of bed, and bandages were applied around the thigh and pelvis; he could now stand perfectly upright, so as to walk with his heel on the ground with the aid of crutches, and from exercise, he grew so rapidly stronger, that on the twenty-fifth day he left off both crutches, and in a month he could walk without a stick; when in five weeks, having particular business, he walked twenty miles, perfectly upright and without the least limping."

"S. NORT."

"In dislocations which have been long unreduced, the bone does not usually snap into its socket at its reduction. A. C."

*Case of dislocation of the right femur downwards, or in the foramen ovale.* "Mr. Thomas Clarke, a farmer, about 50 years of age, was driving home in his cart from market, when the horse took fright and ran away with him; the following is the account he gives of the manner the accident happened. In his endeavour

to stop the horse, he fell over the front of the cart on his face, and the knee struck against some part of it in the act of falling, by which means the thighs were separated, the wheel, he also states, passed over his hip. My friend Mr. Potter in Essex, was called in three weeks after the accident, and I had the pleasure of accompanying him. The nature of the accident was extremely evident, the limb was fully *three inches longer than the other*, the *body bent forwards*, and the *knees separated*, the *foot rather inclined outwards*; these were the leading diagnostic marks.

“ In the reduction, the following were the means employed: Our first object was to produce relaxation, and finding the patient was sufficiently strong to bear the plan recommended in cases of dislocation where much resistance is expected, we therefore drew away some blood from the arm; this however was not sufficient for our purpose, and having carried with us a solution of tartar emetic, it also was administered. The patient was laid upon his side close to the edge of the bed, a girt was passed around the pelvis, carried through the frame of the bedstead, which completely prevented the possibility of the body moving, whilst extension was going on; a second girt was applied between the thighs (fixed to the one above) to which the pullies were attached. Whilst extension was making, Mr. Potter took hold of the limb at the knee, and drew it rather upwards, and towards the sound thigh, occasionally rotating the limb. When the extension had been kept up about ten minutes, the nausea produced by the tartar emetic was so excessive, that the patient begged of us to desist until to-morrow, observing ‘ he felt so bad, that he was fearful of falling off the bed;’ this exclamation, it hardly need be said, was a stimulus to our proceeding, and in five minutes after, the limb was suddenly heard to snap into its original cavity. The patient was put to bed, a roller being applied around the pelvis; at the end of five days he felt so well that he left his room, and at the expiration of a short time, suffered no more inconvenience than a slight stiffness at the joint.”

“ J. S. DANIEL.”

Notwithstanding the dislocations from the acetabulum have



been long ago so minutely investigated, Mr. Cooper has brought our profession under a great obligation, by the detection of a species heretofore almost entirely unknown, and never understood. We allude to the dislocation into the ischiatic notch.

“A robust muscular man, aged 33, was admitted into Guy’s Hospital the 31st of July, 1819. While carrying a bag of sand on the 24th of June, he slipped and dislocated the left hip joint, and the following account he gives of the accident. The foot of the affected side was plunged suddenly into a hollow in the road, which turned the knee inwards, when his body fell with violence forwards. It was found upon examination after he had been admitted, that the thigh was dislocated backwards into the ischiatic notch; *the limb was a little shortened, the knee and foot turned inwards, and the toe rested on the ball of the great toe of the other foot; the head of the bone could not be felt; the trochanter major was opposite the acetabulum, the rim of which could not be distinctly perceived. When the body was fixed, the thigh could be sufficiently flexed to nearly touch the abdomen,*” &c.

The following case was communicated to Mr. Cooper by Mr. Watts, one of the dressers of St. Thomas’s Hospital. “James Hodyson, a sailor, aged 38 years, was admitted into St. Thomas’s Hospital on the 8th of February, 1820, for an injury which he had received in his left hip; his foot was raised from the ground upon a chest of fruit, when another fell upon his thigh, striking the knee inwards; he fell, and being taken up extremely hurt, was directly brought to the hospital. Upon examination, I conceived that it was a dislocation of the hip joint, and that the head of the bone was thrown into the ischiatic notch. Some difference of opinion, however, arose upon the subject, and as considerable tension existed, which prevented the head of the bone from being distinctly felt, I ordered an evaporating lotion, and left the case for future investigation. Upon further consideration, my opinion was strengthened concerning the nature of the injury, as it was clearly pointed out by the *slight shortening of the limb, and the inversion of the foot*, although there was in this case more *power of flexion in the limb* than might have been expected, but *no rotation outwards*. Mr. Cline saw it on the 12th, and thought it a disloca-

tion into the ischiatic notch. 14th. The swelling had greatly subsided; I thought I could now feel the head of the bone in the rotation of the limb. Mr. Cooper very kindly saw it in the evening, and immediately declared it to be a dislocation into the ischiatic notch, and upon his rotating the thigh, I could much more distinctly than before feel the head of the bone in the notch. Feb. 19th. Mr. Chandler thought the swelling had sufficiently subsided to justify the attempt at reduction; and I accordingly made preparations for it in the following manner. At half past 2 o'clock, I took 16 ounces of blood from him; this produced no sensible effect; at 10 minutes past 3 o'clock, I took 27 ounces more, and while the blood was flowing, gave him a grain of emetic tartar; this I repeated till he had taken 5 grains at the interval of a few minutes, and as he was becoming faint, he was taken into the theatre. I applied the bandages and pullies to the pelvis, and to the knee, *bringing the thigh over the other*; we kept up the extension about 10 or 12 minutes before we used a strap to raise the head of the bone, and until I thought it had made some progress towards the acetabulum. We then continued the extension, gradually increasing it, at the same time endeavouring to raise the head of the bone, and turning the knee outwards for about 15 minutes. I had now lost the head of the bone, but still as it had not made the usual noise in its reduction, the extension was continued some time longer, when the bandage at the knee growing loose, the limb was found to have recovered all its motions. Mr. Watts naturally expresses surprise that the bone was reduced without its entering the acetabulum with the usual noise, but when the muscles have been some time contracted, and when the patient is rendered faint by bleeding, and by the nausea of tartarized antimony, they do not act with the same violence as in the first few hours after the accident."

In recapitulating his cases, Mr. Cooper says, "it appears then that in 15 cases of dislocation of the thigh, nine were thrown upon the *dorsum ilii*, four in the ischiatic notch, and two in the *foramen ovale*." We have given these free extracts, because we do not believe that a subject generally considered dull and per-



plexing, can be rendered so interesting in any other mode of discussion.

Of the fractures of the neck of the thigh bone, our author has given too minute elucidations to suffer a hasty analysis; but we can only pretend to notice his most striking observations. His experience proves, what indeed from the natural situation of the parts might have been anticipated, that these accidents are much more common than dislocations from the acetabulum. "This fact is evinced from the comparative number we admit into our hospitals, being seldom without an example of the fractured neck of the thigh bone, whilst the cases of dislocation upon an average do not exceed one in a year." We have always been surprised that so many surgeons could declare themselves convinced of the opposite opinion; and can only account for it by supposing their decisions to be drawn from much more limited observation.

He divides this fracture into two species; first, that in which the bone is broken transversely through the cervix *within the capsular ligament*; and secondly, when it is fractured *externally to the ligament*, either through the root of the cervix, or the trochanter major. "The former of these may be called the *internal*, the latter the *external* fracture, as regards the relative situation of the capsular ligament." The distinctive marks between these, are quite novel and interesting. In the first species, the muscles drawing upwards and backwards the shaft of the femur with that portion of the cervix which remains attached to it, will put the capsular ligament upon the stretch, and thus the shortening of the limb will be limited to the space of one or two inches. The foot will be everted, and the outer portion of the cervix resting upon the brim of the acetabulum will prevent rotation inwards. The pain will not be great, for the rough broken extremity, confined in the cavity of the joint, cannot irritate the neighbouring muscles. Crepitus can only be felt when the limb is drawn downwards and rotated inwards; while in the second species, the crepitus is perceived in almost every motion, the pain is also much more severe, and rotation of the limb can be easily effected; the trochanter in the external fracture is thrown towards the spine of the ilium, does not project so much from the hip, and when the

patient is in a sitting posture, it is lodged in the groin. Another important diagnostic results from the fact that the former occurs from the slightest accidents, while the latter is produced by the utmost severity of violence. Age also affords a distinction; for the internal fracture seldom happens but at an advanced period of life, and the external is received by the most robust individuals. Ossific union, according to Mr. Cooper, eventually becomes another diagnostic. In an internal fracture, the parts can never be united by ossification, while the other species institutes the ordinary process of restoration. To this opinion, however, cannot be conceded the merit of entire originality. When the fracture is altogether internal to the capsular ligament, the separated portion becomes connected to the body by the round ligament alone, and it will require but little anatomical sagacity to determine upon the consequence. Such a paucity of fluids will circulate through the fragment, that the vessels of the broken surface cannot support the exertion of ossific secretion. In consequence of the great mobility of the articulating surface in the cavity, it will be impossible to confine the two portions together, and thus another serious obstacle will be opposed to such a union. There has never, however, been any unanimity of sentiment upon this subject: and, perhaps, we can account for the opposite opinions that have been entertained by different authors, from the fact that no systematic discrimination has been made between the two species of fracture. To Mr. Cooper belongs the high credit of having elucidated this intricate subject, and after the results of his observations and experiments, we may indulge but little hesitation in our judgment. Besides the opportunities which he improved of examining the injured parts after death, he performed several experiments upon animals; all of which demonstrated, that although ligamentous matter was formed between the fractured surfaces when kept in close contact, no real ossification ever commences. In managing these fractures, he advises to place the patient upon his back on a mattress, and to support the thigh and leg, with the knee bent, over a double inclined plane, with a splint extending from outside the knee to above the hip; the femur is then confined by a roller



buckled firmly around the pelvis, so as to press the one portion of bone upon the other.

In oblique fractures of the shaft of the femur, Mr. Cooper uses the same method, and thinks it superior to the apparatus of Des-sault. But we can by no means agree with him on this point. Successful experience seems now to have convinced the generality of our surgeons, that the French apparatus, when skilfully applied, is altogether to be preferred to any such contrivance. It is a very uncommon thing that we meet with an individual exhibiting any deformity after having a broken thigh bone managed by the permanent extension: whereas it is a well known fact, that in the London hospitals, where the flexed position of the limb is adopted, patients are rarely dismissed without a limping in their walk. Unless the whole extremity is confined between the long splints to the pelvis, we cannot conceive how any position will prevent every motion of the trunk from distorting the apposition of the fractured surfaces. Nothing can oppose the retraction of the lower portion so effectually as the gradual relaxation of the irritated muscles which long continued extension produces; and we feel, therefore, no hesitation in commending the choice of our American practitioners.

In the management of a fractured cervix, however, the plan recommended by Mr. Cooper appears very reasonable, and may possess some advantages over the method of Dessault. It is not so complicated, and can be applied with more facility. It promises also, to act as immediately on the hip; while the position of the trochanter, and that portion of the cervix which remains attached to it, may be more extensively varied.

Under the head of dislocations of the Patella quite an amusing method is described, which certainly improved all the advantages of muscular relaxation that a flexed extremity could afford. The surgeon placed the patient supine, and elevated the affected limb by placing its heel over his shoulder. With both hands then at command, he pushed the patella into its natural situation from whence it had so pertinaciously escaped, and the *lady* walked soundly away. We, however, would not be understood to recom-

mend such a practice in this country under the same circumstances.

In his discussion of fractures of the patella, our author has fully occupied the subject of ligamentous union; and his experiments have fairly demonstrated, what indeed was not before unknown, that complete ossific junction rarely occurs between the separated fragments. In longitudinal fracture of the patella only one of his experiments succeeded in obtaining even an incipient ossification, and but two cases are recorded where this union was supposed to be effected. In the transverse fracture he decides that the union is always ligamentous. A curious circumstance is recorded in this part of the subject, which may throw some light on the physiology of ossification. "The bone itself undergoes but little alteration; the lower portion joined by ligament to the upper, has its broken cancellated structure still apparent, although a little smoothed. The upper portion of bone has its broken cancelli covered by a slight ossific deposit, so that there is more ossific action in the upper than in the lower portion of the bone, and certainly much less than in bones which do not form a part of the joints." When we recollect the relative situation of the two fragments, this circumstance will be easily understood, and it will be found to concur very aptly with the explanation we have given of the consequences of a broken cervix femoris. The lower portion, in a transverse fracture of the patella, receives a scanty circulation through the ligament which unites it to the tibia alone; while the upper fragment is much more abundantly supplied from the arteries of the extensor muscles. Thus the vessels of the superior half are enabled to throw out ossific particles to fulfil the ordinary purposes of restoration, while the capillaries of the inferior portion are just able to eke out their proportion of adhesive ligamentous matter.

Of the internal derangements of the knee joint, about which Mr. Hey speaks so indefinitely, our author seems to have formed a precise opinion; and denominates them *partial luxations of the thigh bone from the semilunar cartilages*. "The semilunar cartilages which receive the condyles of the os femoris are united to the tibia by ligaments, and when these ligaments become relaxed



and elongated the cartilages are easily pushed from their situation by the condyles of the os femoris, which are then brought into contact with the head of the tibia, and when the limb is attempted to be extended the edges of the semilunar cartilages prevent it. How then is the bone to be again brought upon the cartilages? Why, as Mr. Hey has advised, by bending the limb back as far as is possible, which enables the cartilage to slip into its natural situation, from the pressure of the thigh bone being removed in the bent position, and the leg being brought forwards it can then be completely extended, because the condyles of the os femoris are again received upon the semilunar cartilages." "The most frequent cause of the accident is from a person in walking striking his toe when the foot is everted against any projection (as the fold of a carpet); he immediately feels pain in the knee, which is unable to be completely extended. I have seen this accident also happen from a person having suddenly turned in his bed, and the clothes not suffering the foot readily to turn with the body, the thigh bone has slipped from its semilunar cartilage. I have also known it occur from a sudden twist of the knee inwards when the foot was turned out."

When we consider some circumstances mentioned by Mr. Hey, this theory will appear still more plausible. He declares that the ligamentum patellæ always appeared more relaxed than its fellow on the opposite limb; and that the patient can never flex and extend the leg himself without suffering pain; whereas no passive motion produced by the surgeon was attended with inconvenience. In this derangement the articulating surface of the condyles will not be separated from the tibia by the interposition of the semilunar cartilages, and as a sure consequence the ligament and muscles of the patella must become somewhat relaxed. The contraction of the muscles of the thigh, also in active flexion and extension, will force the opposite surfaces of the articulation together, and thus contuse the misplaced cartilages and ligaments; while passive motions, performed even without the necessity of friction, excite no disagreeable sensations.

It seems, however, that the treatment just mentioned has not always succeeded. Even Mr. Cooper, in one instance, was alto-

gether baffled, as was the venerable Mr. Hey himself, to whose care he had sent the unfortunate patient; an example of humanity and magnanimity, as well as of professional courtesy—one which we should be gratified to see more frequently exhibited among our own professional brethren. Another mode of reducing dislocated semilunar cartilages, perhaps more scientific than the former, is detailed in the following case:—"Mr. H. Dolbey, æt. 37, has often dislocated his knee by turning the foot inwards and the thigh bone outwards, by accidentally slipping in walking on uneven ground, or under sudden exertions of the limb, considerable pain is immediately produced, accompanied with considerable swelling. His mode of reducing it is as follows: he sits on the ground, and then bending the thigh inwards and pulling the foot outwards, the sub-luxation of the os femoris being external, the natural position of the limb becomes restored. A knee cap laced tight around the knee is the usual preventive of the return of this accident, but it is not sufficient in Mr. Dolbey, without the addition of straps, and more especially of a very strong leather one just below the patella."

On the dislocations of the ankle joint there is a very long disquisition; but, although every part of the subject is made quite interesting, we have room to notice only his management of the compound species. According to our opinion the treatment of this terrible accident has in this country too generally involved amputation; and we cannot avoid expressing some gratification in informing our readers of Mr. Cooper's concurring experience. It has been inculcated as a precept in his lectures for several years past, always to attempt the preservation of the limb after a compound dislocation of the ankle; and many successful experiments are now communicated from his various correspondents. When the luxated bones are not greatly protruded or comminuted, he recommends to replace them in apposition; to cover the wound in the integuments with lint dipped in blood, which by coagulating affords the best external application, and to support the parts by the many-tailed bandage and excavated splints. In cases, however, where the protrusion is very great, or the bones are much splintered, he advises to saw off the projecting portions, and to



remove the comminuted fragments. "If the dislocation can be easily reduced without sawing off the end of the bone; if it be not so obliquely broken but that it remains firmly placed upon the astragalus when reduced; if the end of the bone be not shattered, for then the small loose pieces of bone should be removed; and the surface of the bone be smoothed by the saw; if the patient be not excessively irritable, so as to occasion the muscles to be thrown into violent spasmodic actions in the attempt at reduction, and which leads to subsequent displacement when the limb has been reduced; the bones should be at once returned into their places, and the parts should be united by the adhesive inflammation; but rather than amputate the limb, if the above circumstances were present, I should certainly saw off the ends of the bones."

In the enumeration of the advantages of removing the ends of the bones, one observation is made, of which we wish those physiologists who deny all vitality to the cartilages to be reminded—"and that adhesive matter can be thrown out upon cartilaginous surfaces, is known to every person who has dissected a diseased joint." Let this be thrown into the same argument with the fact that the cartilaginous portions of the joints have frequently been found altogether absorbed, leaving the articulating surfaces with a polished bony appearance, and we shall not require more evidence to confute such an unphilosophical opinion.

The volume closes with two shorter essays by Mr. Cooper: *one on unnatural apertures in the urethra*, another *on encysted tumours*. In the former much novel information is detailed respecting the management of obstinate perineal fistulæ. For the ordinary cases which result from strictures, he recommends to commence with the usual bougieing practice; and as soon as a full sized catheter can be worn, to attempt the obliteration of the opening, either by excising the callous edges for the purpose of procuring adhesion, or by exciting the granulating process around the fistulous orifice. To fulfil this indication by the latter method, he found the nitrous acid most effectual, and this should be repeatedly applied, so as to form a granulating surface both in the fistula and over a considerable space around its external aperture.

There is, however, one form of this disease, of which we do not recollect to have noticed any description before: in which this treatment becomes altogether inadmissible. "Apertures are sometimes formed in the urethra from a process of ulceration beginning in a bad constitution, without their being accompanied by stricture. A person whose constitution is broken by excess, or who is naturally feeble, will have a slight discharge take place from his urethra without any previous sign of disease, or without the possibility of a suspicion of gonorrhœal infection; a swelling forms in a line with some part of the urethra, which proceeds to suppuration: a poultice is applied, and the abscess breaks, or is opened by art, and the urine takes its course through the wound. These circumstances arise either from ulceration in the mucous membrane of the urethra, or from abscesses in the lacunæ; and I believe more frequently from the latter than the former. It is the usual practice in these cases, to begin directly with the use of bougies, but it is not judicious to do so, as they only add to the tendency to ulcerate, and increase both the local and constitutional irritation." Two cases are related by way of illustration; one of which was cured by poultices and tonics, while the other proved mortal from the sloughing excited by the irritation of bougieing. As this species is not unfrequently met with in our large public institutions, and the treatment commonly resorted to being very hazardous, we hope that it will not hereafter escape observation.

A very interesting case of a fistulous opening between the commencement of the urethra and the rectum, is related in this essay, in the management of which much originality is conspicuous. It is stated that wearing a very large catheter, (which, by completely filling up the urethral canal, prevents all escape of urine around it,) will frequently excite a contraction and cicatrization of the fistula. In the case alluded to, Mr. Cooper first resorted to this plan, and ordered his patient at the same time to lie in bed. But after a full trial, the urine still continuing to flow from the rectum, the following novel operation was contrived and executed. A staff being carried into the bladder, an incision was made through the perineum exactly as in lithotomy; and when the



membranous part of the urethra was exposed, the knife was carried between the prostate gland and the rectum, so as completely to cut across the fistulous canal. A piece of lint was introduced into the wound, and a poultice was applied over it. "When the lint was removed, the urine was found to take its course through the opening in perineo; the aperture in the rectum gradually healed, and that of the perineum quickly closed, after which the urine took entirely its natural course."

After effusions of urine behind the perineal fascia, so large a surface of the integuments, and indeed of the spongy texture of the urethra itself, will sometimes slough away, as to render all the ordinary methods of treatment abortive. The opposite edges of the exposed surface cannot be brought sufficiently in contact to maintain the adhesive process; nor can granulations be made to stretch across such an extensive aperture. Few would suppose a case of this kind remediable by any exertion of skill; and our readers, perhaps, will pause before they can believe even the magnus apollo of his profession successful. But it is not the character of Mr. Cooper to succumb to common impediments; and he accordingly seized this opportunity to raise another tribute for the posthumous glory of Taliacotius. "As there appeared no means of relief from this distressing condition except by surgical operation, and from the extent of the wound, bringing the edges together either in the transverse or longitudinal direction, offered very little chance of success, Mr. Cooper proposed to supply the deficiency by a covering of integument from the scrotum. With this view the following operation was performed. An elastic catheter being passed into the bladder, the callous edges of the opening were pared off, so as to produce an entire new surface; a portion of integument was then dissected from the scrotum, (leaving it attached at the upper part) and turned over upon the wound, to which it was held down by four sutures covered by small strips of adhesive plaster; a bandage was applied to support the scrotum, and the patient placed on his back in bed." Though there was some difficulty experienced in procuring a complete adhesion of the flap over the fistulous surface, yet the patient, from the history of whose case this extract is made, was perfectly relieved, and

may long live to thank his benefactor for this new application of the once ridiculed Taliacotian art.

In speaking of the paper on encysted tumours, which closes the volume under consideration, we really cannot venture to compliment Mr. Cooper. The opinion he entertains of their formation, though no doubt correct in many instances, is altogether too general to sustain an application to our experience. We have frequently removed subcutaneous encysted tumours which maintained no such connection with the skin as could warrant the supposition of their having originated in obstructed follicular ducts. But the idea is certainly ingenious, and may give rise to a classification of these hollow cysts into two species: one produced, as our author says, by an enlarged adipose follicle; another arising from some morbid process in the cellular tissue. The common method of dissecting out these tumours, has always proved in our hands exceedingly troublesome; and we are much pleased with a plausible suggestion which this paper contains. He recommends to make a free incision through the integuments into the cavity of the tumour, and then if the cyst does not become everted by pressure, as it generally does, to draw it out by the forceps and dissect the whole away by a scalpel. In attempting to dissect around a sac of this kind, underneath the superciliary ridge, we once unintentionally cut directly into the cavity; and the value of this suggestion was then ascertained, from the facility with which the parietes were extirpated.

In concluding our review of this interesting publication, we cannot avoid expressing to Mr. Cooper, what we know will be the sentiments of the whole profession;—our sense of obligation for what he has already done, and our earnest wishes that he will continue to favour us with many more such communications. His redundant and ill constructed sentences, though they are very faulty, will all be excused, while they convey such sound practical information, and indicate such an active and independent judgment. But we will not attempt to praise Mr. Cooper, for he has raised himself to a character above all common eulogy. M.



## ANALECTA.

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### *Fracture of the Fibula.*

MR. DUPUYTREN has lately published an important paper,\* on the character and treatment of fractures of the lower end of the fibula.

He thinks it of great importance, to attend to the distance from the lower extremity at which the fibula is fractured. When the fracture occurs within three inches of the malleolus externus, it is generally attended with dislocation of the ancle, or if this does not exist, it is extremely apt to occur when the patient makes any attempt to walk. If the fracture of the fibula occurs *more* than three inches from its lower extremity, there is no chance of a dislocation of the ancle taking place, as the *tibio-peroneal* ligaments are preserved. The fibula, though liable to fracture in every part of its extent, is most generally broken, according to Mr. Dupuytren, about two and a half inches from the lower end of the bone. At this point it is weakest, and has a natural bend inwards, from the constant action of the muscles.

Mr. D. thinks that the luxation of the foot *backward*, which sometimes attends fractured fibula, or, what is the same thing, where the tibia is driven forward toward the instep, is always the effect of the action of the *gastrocnemii* muscles, and not of the same force which has produced the fracture of the fibula. Such luxations are always incomplete; and the malleolus internus remains unbroken. If the internal malleolus is fractured, the dislocation is often very great; the heel is longer, and a hard tumour projects in front of the foot. In common luxations of the foot the external malleolus is carried forwards along with the tibia and fibula, and forms a prominency like that of the internal malleolus; but in the luxation attending a fracture of the fibula, it is drawn backward with the foot, to which it has a firm attachment by the lateral ligaments.

When the fibula is broken above three inches from its lower extremity, and without being displaced, nothing but *rest* is requir-

\* *Annuaire Medico-Chirurgicale des Hospitaux.*

ed for its cure. In fractures which are situated *within* three inches of the malleolus externus, "perfect rest and security from motion" are imperiously necessary, and even where there is no dislocation of parts, in order to prevent such an accident. When simple dislocation occurs with fracture of the fibula, it should be *immediately* reduced; and this becomes the more necessary when "the fracture and dislocation are accompanied with rupture of the ligaments, separation of the malleolus externus, fracture of the tibia, extravasations of blood, wounds of the integuments, &c." Mr. D. is of opinion that the dislocations should be *immediately* reduced, at whatever period of the accident we may be first called to the patient.

Mr. Dupuytren's apparatus for dislocated ankle joints and fractured tibia, appears to us extremely well adapted for its purposes, and is withal of such "simple mechanism,—is composed of such common materials, and is of such easy application, that it presents no difficulty even to the most inexperienced tyro in surgery."

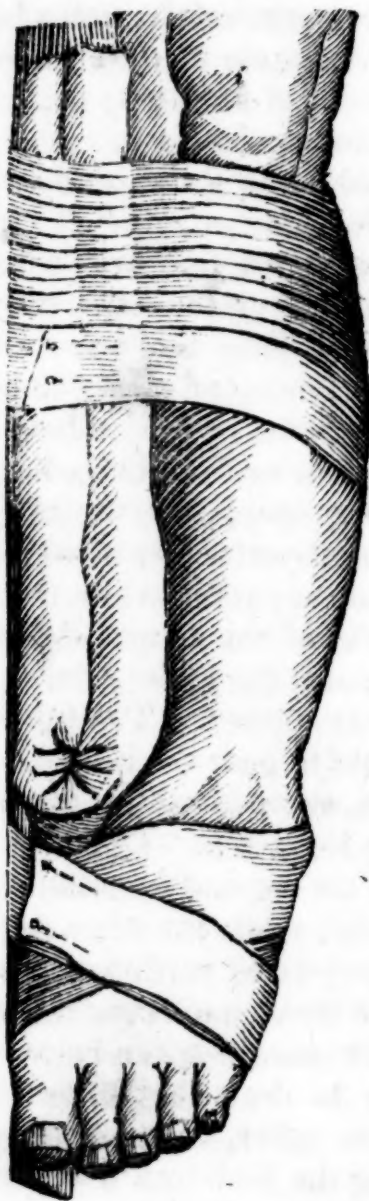
It consists, of a cushion, a splint, and two rollers of bandage. "The cushion, made of linen, and about two-thirds filled of cotton, should be about two feet and a half long, four or five inches broad, and three or four inches thick. The splint, from eighteen to twenty inches long, two and a half inches broad, and three or four lines in thickness, ought to be made of pretty firm wood, and but little elastic. The rollers, of half worn linen, should each be about four or five ells in length."

"The cushion being doubled upon itself, lengthwise, forms a kind of wedge, the thicker extremity of which is to rest on the malleolus internus, but not to pass it, while the other extremity comes on the internal condyle of the femur, thus furnishing a complete defence to the leg against the splint, and a *point d'appui* for the latter when the roller is applied. The splint then being laid along over the cushion, ought to pass the inferior extremity of the latter from five to six inches, so as to project three or four inches beyond the inner edge of the foot itself. One of the rollers is now to be neatly wound round the leg and apparatus, below the knee, so as to secure it at that point, while the other roller being passed twice or thrice round the projecting portion of the splint below, so as to take hold on it, is to be wound from thence round the heel and instep alternately, the vacant space between the splint and foot enabling the surgeon to draw the latter towards the former by figure of 8 turns of the roller, with any degree of force that may be necessary to bring the foot into a proper line of bearing with the leg, and retain it there. By this apparatus, not only the foot is drawn inwards to its proper place by the roller going round the projecting splint as its lever, but the tibia and astragalus are



pressed outwards by the base of the cushion, which forms the *grand point d'appui* of the whole apparatus. Thus, also, the lower fragment of the fibula being pressed *outwards* by the tibia above, and drawn *inwards* by the lateral ligaments below, exercises an action on the astragalus the reverse of that which displaced it, and consequently retains it in its natural situation. The limb is now to be half bent, and laid with its outside resting on a pillow, when the patient will generally express himself as being very easy and comfortable.\*

In order that our readers may easily understand the above description of Mr. Dupuytren's apparatus, we annex the following wood-cut, taken from an engraving in the London *Medico-Chirurgical Journal*.



\* We have condensed this article from an interesting Analytical Review of Dupuytren's paper, in the June (1820) number of the London *Medico-Chirurgical Journal*.

There is often much difficulty in retaining the parts *in situ*, after the reduction of a dislocation of the foot backwards. Mr. Dupuytren found the above apparatus very applicable and effectual. In such a dislocation, after it is properly reduced, the long cushion is to be placed *under* the limb along the gastrocnemii muscles, reaching from the heel to the ham—the base or thicker end being below, and the thinner extremity above. The splint is laid over this cushion, and a roller applied round the leg and apparatus at its upper end. A small cushion, being a few inches square, is next placed on the fore-part of the leg, close to the instep, and a roller passed round it and the lower part of the splint, “with such a degree of tightness as is thought necessary.” “It is evident that the action of this roller is to press the heel forward, and the tibia backward at the same time.”

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*Stricture of the Rectum.\**

It is sometimes extremely difficult and even impossible, in cases of stricture of the rectum to introduce the rectum-bougie. An ingenious mode of doing this has been devised by Mr. J. M. Coley, of Bridgworth, England. Mr. Coley had a case of stricture of the rectum under his care, in which all attempts to pass a bougie were found impracticable, “in consequence of the oblique course of the stricture, the projection of the upper part of the sacrum and a thickening of the intestine towards the bladder of such extent as to occasion most distressing strangury and hæmaturia.” Other surgeons were called in consultation, and it was decided that the introduction of the bougie into the stricture was impossible. A sponge-tent was then tried, but this also was found to be of no avail. In this failure of all the ordinary resources of our art, it occurred to Mr. Coley, “that the bougie might be defended from all intervening obstructions, by means of a tin canula, and thus prevented from bending upon itself; a circumstance which had hitherto foiled his endeavours.” A bougie of a size that would just enter an adult urethra, was readily introduced into the stricture by means of such a canula. This operation was daily repeated with a gradual augmentation of the size of the bougie and canula, and thus in the course of two months the bowel was dilated as much as was necessary. “By these means, and the administration of pilulæ hydrargyri and aperients, the patient’s

\* Observations on Strictures of the Rectum, &c. By W. White, Esq. 8vo. pp. 172. London. 1820.



health was restored, and his usual bulk regained." Without the assistance of the canula, the bougie could never be passed into the stricture, throughout the whole treatment of this case. "When the diameter of the canula was increased, to admit a larger bougie, Mr. Coley found, that by holding a lighted candle at the lower end, the orifice of the contracted part of the intestine could be readily distinguished."

The bougies Mr. C. uses, have a loop at the lower end, about three inches long, for the purpose of being secured to a T bandage. The points of the bougies ought to be conical. He passes them into the rectum so as to be *wholly concealed within the bowel*; this he considers as of great consequence, "as it enables the patient to walk about, or even to ride on horseback." They should, he thinks, be left in the bowel all night, if possible, which he supposes will promote the absorption of diseased structure, by the long continued pressure, and resist the tendency in the constricted parts to contract. "At the same time he observes, that the discharge of the cerate (of the bougie) produced by the heat and moisture of the anus is avoided; the cerate *not being melted by any portion of the intestine above the sphincter.*"

The canulæ employed by Mr. C. are of three different sizes. They are six inches long, cylindrical, turned down at each end, and below the upper extremity, perforated with holes in a row, within one sixth of an inch from the margin. "A small piece of wash leather is to be bound round the edge through these holes for the purpose of defending the inner coat of the intestine during its introduction. A piston of wood rounded at the top, extending half an inch beyond the canula, and sliding freely within it, with a handle and top at the bottom, should be in readiness. The diameter of the smallest canula must be half an inch, of the next size seven-eighths of an inch, and the largest one inch and an eighth. The pistons should be adapted to the different canulæ. The most favourable position for the patient is the same as for most other operations about the anus; the body should be bent forward while the head and hands rest on the seat of a chair. The piston being placed with its corresponding canula, and the outside of them both being covered with butter, or fresh lard, they are to be gently introduced within the anus, the sphincter of which must also be anointed. The piston being withdrawn, the canula is to be pushed upward until it arrives at the stricture, which will be known by the resistance experienced. By the pressure of the canula against the stricture, its lips are expanded, and the rugæ of the mucous membrane removed. The bougie should now be conveyed through the canula into the orifice of the stricture, when its progress will generally be suspended by a sudden spasm which

is involuntary and imperceptible to the patient. During this irritable state no attempt must be made to carry forward the bougie. Having waited a few minutes, it may be pushed onward in a gentle manner, while the lower end of the canula is raised towards the apex of the os coccygis, and inclined to either side according to the course of the stricture. The length of the bougie need not exceed eight inches, which being deposited in the situation that will presently be explained, will be equal in effect to one of ten inches employed in the usual manner. When it has fairly passed beyond the obstruction, it should be propelled further until its lower end has entered the canula to the distance of three or four inches, which may be effected by the assistance of the piston. While it is retained at this elevation by the piston, the canula must be drawn down to the top, when the bougie will escape from the apparatus and lie across the pelvis, resting its lower end against the os coccygis or the levator ani, between that part and the tuberosity of the ischium. Lastly, the canula and piston are to be withdrawn.\*

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In the October number of the London Medical and Physical Journal, there is a very remarkable case stated by Mr. Young of the beneficial effects of his practice of pressure in Cancer. The patient had been successively under the care of Sir Wm. Adams, Mr. Astley Cooper, Mr. Henry Earle, Mr. Wordrop, and several other surgeons of inferior note, without obtaining any relief.

In the same number there is recorded by R. W. Bamfield, Esq. surgeon, a case of a very old and irreducible entero-epiplocele, with adhesion to its sac, that was cured in three weeks by confinement to bed in the horizontal position, keeping the bowels free from constipation and the frequent employment of the taxis.

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*Antidote for Vegetable Poisons.*

M. Drapiez has ascertained by numerous experiments that the fruit of the *fewillea cordifolia*, is a powerful antidote to the vegetable poisons. This opinion has long been maintained by naturalists, but has not before been verified by experiments M. Drapiez poisoned dogs with the *rhus toxicodendron*, (poi-

\* Dr. Johnson's Medico-Chirurgical Journal, September, 1820.



son oak), hemlock, and nux vomica. All those that were left to the effects of the poison died; but those to whom the fruit of the *fewillea cordifolia* was administered, recovered completely, after a short illness. To see whether this antidote would act in the same way, when applied externally to wounds into which vegetable poisons had been introduced, he took two arrows which had been dipped in the juice of manchenille, and slightly wounded with them, two young cats. To one of these he applied a poultice composed of the fruit of the *fewillea cordifolia*, while the other was left without any application. The former suffered no other inconvenience except from the wound, which speedily healed, while the other in a short time fell into convulsions and died.

It would appear from these experiments, that the opinion entertained of the virtues of this fruit in the countries where it is produced, is well founded. It would deserve in consequence to be introduced into our pharmacopœias as an important medicine; but it is necessary to know that it loses its virtues if kept longer than two years after it has been gathered.

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Dr. Mongery, physician to the hospital of the colony of Ferdinandina, in the island of Cuba, states, that he has discovered a new remedy for the tape worm, of extraordinary efficacy. It consists in giving to the patient, fasting, three ounces of a paste made of fresh *citronille*,\* and afterwards six ounces of honey, in three doses; the first at the end of one hour, and the others after similar intervals. By this process, Dr. M. says, the *tænia* has been always expelled after the lapse of six or seven hours, and in many of these cases all the medicines of most reputed efficacy have been tried without success. The worm is voided knotted and rolled up in a ball, and not in fragments as is commonly the case when it is expelled by other anthelmintics. On subjects who had two of these worms they were both expelled at the same time in the way just described.—*Ibid.*

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Dr. Perret has communicated to the Physical Society of Corcaponne, a series of observations in proof of the efficacy of the

\* Neither the editor of the London Medical and Physical Journal, nor the editor of the "Journal Universel des Sciences Medicales, to whom the account of the remedy, was originally sent in a letter, know precisely the plant designated by Dr. M. It has been conjectured by some that it is *citronelle*, (balm, melissa. Lin.) by others it has been supposed to be the fruit of the *cucurbita citrullus*, by others the *cucurbita pejo*, (the common pumpkin or gourd.)

plantain root in intermittent fevers. It has long been employed in such cases by the German Physicians.

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Laceration of the perinæum is seldom fatal, yet perhaps there are few accidents in midwifery, which in their consequences, expose the patient to a greater variety of inconveniences than this. We think practitioners have been too much disposed to leave this *accident to nature*, a poor apology for imbecility and indolence. The following short extract from a case stated by Mr. Thomas Alcock, surgeon, accoucheur to the St. James's Infirmary, will show that a little exertion may save the patient all these disastrous consequences.

"Mrs. F. was delivered of her first child, which was large, without professional assistance. A laceration of the perinæum to within half an inch of the anus was the consequence. At the end of a week a thin slough was thrown off from the lacerated edges, leaving the ulcerated parts clean and florid. Mr. A. instead of abandoning his patient to her fate, as is usually done, placed the ulcerated edges accurately in apposition and detained them by a suture passed midway between the extremities of the lacerated portions; the knees were kept together by a slight bandage, and strict attention to cleanliness observed. In three days the ligature was removed, and union from the point where it was inserted was perfect posteriorly. Another ligature was then passed within a few lines of the most anterior portion of the ulceration, and in three days more the union was complete and the perinæum restored to the same state as before the accident." Mr. Alcock thinks that hare-lip suture might be employed in this accident with advantage.

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*Antimonium Tartarizatum.*

One fluid ounce of the decoction of yellow bark is capable of completely decomposing ℥j. of tartarized antimony, and rendering it inert. Bertholet has accordingly recommended the immediate exhibition of this decoction, when an over-dose of the salt has been taken. Rhubarb is equally incompatible.

*Dr. Paris's Pharmacologia.*



*Balsam Copaiba.*

Mr. Bucholtz asserts that if balsam copaiba does not dissolve in a mixture of four parts of alcohol and one of rectified ether, we may infer that it is adulterated.

*Gastrotomy—terminating successfully.*

A case is related by Dr. Marechal\* in which M. Carré opened the abdomen of a woman, and removed a tumour weighing seven pounds. The tumour adhered to a portion of the intestines and to the epiploon; it was of a mamillary kind of substance, and part of it had degenerated into a semi-putrid state.

Another case of gastrotomy is related by Dr. Cayrocke,† in which not only the abdomen, but the stomach itself was opened for the purpose of removing from it a silver fork which was accidentally swallowed by a woman 24 years of age. The fork was removed after having lain three months in the stomach, and the patient did perfectly well after the operation.

*Experiments with the Root called Treba Yapan, as a new remedy in Herpetic Eruptions.* By Professor HUFELAND, of Berlin.

Dr. Hufeland was informed by a gentleman who had travelled in the island of *Java*, that the natives of that country were much in the habit of using a root which they call *treba yapan*, in every variety of herpetic eruptions, and with the most remarkable success. Anxious to try this article himself, this zealous and enlightened veteran of our profession obtained some of it through the agency of Mr. *Haken*, the Prussian consul at *Treplaw*, and also an account of the manner in which it is employed by the natives, which is as follows:

The root being cut into small pieces, is rubbed down in a mortar, with good vinegar, into the consistence of a pap. The part occupied by the tetter is to be smartly rubbed with flannel, or the

\* Observation d'une Gastrotomie Pratique avec succès à Sedan ; communiquée par M. MARECHAL, Docteur en Médecine. Seance, du 16th Nov. 1819, Société de Médecine.

† Operation de Gastrotomie, par M. Cayrocke, M. D. à Mendes. Journal general de Médecine, Janvier 1820.

bare hand; immediately after which, the pap formed of the root is to be applied over the whole diseased spot, to the thickness of about one half a line, and left to dry for several hours. This must be repeated 6 or 8 times, (twice a day.) It is asserted that it has never been known to fail.

In the hospital Charité, of Berlin, the following trials were made with this root, under the superintendence of Dr. Neuman.

1. J. V—, aged 19, was admitted into the institution, affected with the itch, and a tetter on the back of his right hand. The tetter had existed for several years, and had until then resisted all attempts to remove it. The itch was but recently contracted, and soon yielded to the ordinary remedies for this complaint. The tetter, however, which was about the size of a dollar, remained uncured, although powerful applications were made to it: such as a solution of muriate of mercury, petroleum, *unguent. rosismarini compositum*, and a variety of other things.

The <sup>med</sup>ication of the *treba japan* was now applied, in the manner <sup>met</sup> mentioned. After it had been repeated twelve times, the tetter <sup>as ent</sup>ion entirely removed. The patient was kept in the house for some <sup>of</sup> the order to observe whether it would return again, but as no signs of re-appearing was discovered, he was discharged from the hospital as cured.

2. J. S. a patient in the same institution, had a very large tetter on the back of one of his hands. The ordinary remedies were also employed in this instance, without the least advantage. The *treba japan* was then applied, and after having repeated its application eight times, the disease was entirely removed.

3. E. E. aged 20 years. This patient was received into the hospital, affected with itch, and a syphilitic ulcer on the glans penis. The chancre healed in the course of 12 days, under the use of calomel. The itch was obstinate, and yielded very slowly. While under care, a herpetic kind of eruption came out on his cheeks. This was very speedily removed by the *treba japan*.

Dr. Hufeland laments that the smallness of the quantity of this article obtained by him, did not allow him to make any further trials with it. He thinks, however, that he has had sufficient evidence of the power of this root, in the cure of herpes, to entitle it to the particular attention of the profession.

The root has no smell, and communicates a warm, and somewhat acrid sensation to the tongue. Its botanical character has not yet been ascertained.

*Hufeland's Journal der pract. Heilkunde, 1820.*



*Remedy for Bronchocele.*

The Bibliotheque Universelle, for July 1820, contains a paper by Dr. Coindet, on a new remedy for goitre, which from his experience, appears to be very effectual. It is iodine. One preparation was a solution of 48 grains of hydriodate of potass, equivalent to 36 grains of iodine, in an ounce of water. Sometimes iodine is dissolved in this solution, to increase the force of the remedy in very difficult cases.

Another preparation, called tincture of iodine, was made by dissolving 48 grains of iodine in an ounce of alcohol of 35. (S. G. P. 42.)

The quantity for an adult, is ten drops of one of these preparations in half a glass of syrup of capillaire and water, taken early in the morning, fasting; a second dose was given at 10 o'clock; and a third in the evening, or at bed-time. At the end of the first week, 15 drops were given in place of 10, thras opes a day; and, in a few days after, when the effect seemed wasant on the tumours, it was increased to 20 drops. This qu forky has rarely been increased, and was generally sufficient to disnd ate the largest goitres.

After about eight days treatment the skin becomes less tense, and apparently thicker. The tumour softens, as becomes evident to the touch: the goiterous tumour, if there are several, become distinct and separate: they soften and gradually dissolve. In many cases the nucleus, or part which is organically deranged, becomes harder, diminished in size, and isolated: sometimes they become moveable; a circumstance of great advantage in those cases where an operation is necessary.

In some cases the cellular structure which pervaded the tumour, remains swelled, and feels like an empty cyst. Frequently also the goitre disappears only partially, but to an extent sufficient to be neither inconvenient, nor a deformity. In many cases it is dissolved, destroyed, and dissipated, in from six to ten weeks, so as to leave no traces of its previous existence.

*Journal of Sciences, Lond. No. 19.*

We request our medical friends in the western and northern parts of the United States, to try this remedy.

*New Mode of Treating Bronchocele.*

Dr. Quadri of Naples, describes a new and successful method of treating this disease. It consists of passing a seton through the substance of a large gland, with the view of exciting suppuration, and also such a degree of irritation as might stimulate the absorbent vessels to remove a portion of it. The particulars of several cases are related, from which the following practical inferences are drawn.

1st. That it is not dangerous to perforate the bronchocele, carried deep into the gland, provided the needle be not brought near the cartilage.

2d. That the consecutive inflammation is trifling. In the first case, the seton escaped a few days after the operation was performed; when the matter was squeezed out, and the irritation occasioned by replacing it, produced an abscess on the neck, and on being opened, it was found that the suppuration had effected the destruction of nearly the whole gland.

3d. When the irritation of the seton is not adequate to excite the requisite degree of inflammation, it may be successfully obtained by the introduction of a piece of hellebore. 4th. That it was expedient to retain the seton in the tumour for a considerable time, in order to keep up the suppuration until a cure is accomplished, as well as to prevent the formation of sinuses. That the beneficial effect of suppuration rarely extends throughout the whole substance of the gland, but in general only destroys that portion of it contiguous to the perforation.

In the immediate neighbourhood of the part through which the seton passes externally, an irregular fungus margin usually rises up, which must be removed with scissors on withdrawing the seton, to prevent deformity. After suppuration ceases, and the wounds are healed, the curative process goes on, and the bronchocele continues to diminish until it is totally dissipated. The skin which has been distended in covering the tumour, soon resumes its natural appearances.

*Medico-Chir. Trans. Lond. vol. 10.*

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*Zincum Cyanicum.*

Professor Hufeland has employed a cyanite of zinc in various nervous diseases, and he states, with very good success. It may be given in the dose of from one to four grains, two or three times



a day. He did not observe any deleterious effects from the employment of this article. It has a tendency, however, to constipate the bowels, and when carried beyond a certain dose, produces nausea. In cramp of the stomach, he saw this medicine produce the most prompt and happy effects. He derived also great benefit from its employment in a few epileptic cases, and in paralysis.

*Hufeland's Jour. der Pract. Heilkunde, March 1820, p. 106.*

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*Acites and Anasarca cured by Electricity.*

Mr. James Odell, aged 59, states in the Montreal Herald, under date of Feb. 21, 1821, that he had been afflicted with dropsy, for nearly two years: that in July, 1819, he had 7 or 8 quarts of water drawn from his legs, and was afterwards tapped in his abdomen 14 times. In September last, Dr. Carter of Montreal, cured him by electricity. The precise mode of application is not stated. He can now eat, drink, and sleep, as well as at any time for 30 years past.

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In the August number of the Repertory of Arts, &c. there is an interesting paper on preserving anatomical preparations by means of a solution of muriate of soda. The silver medal was voted to Mr. Cook, the author, for this paper, by the Society for the Encouragement of Arts, &c. Lond. We shall make a few extracts for the benefit of our readers.

“The muriate of soda, when dissolved in clear water, and filtered, constitutes a solution as bright as any can possibly be.”

When animal substances have been properly macerated, and put into this solution, it will retain its brightness as long as it is excluded from the air; and will rarely deposit as much precipitate as spirit of wine. It may be used on a large scale, in vessels less accurately closed, upon specimens of any bulk, for occasional inspection, for which the costliness and volatility of spirit almost unfits it. It may be substituted for spirit almost universally, and there are some parts which it preserves in the natural condition, which spirit contracts. Mr. Cook's process is briefly this. The specimen is first well macerated in clear water. After it is perfectly deprived of the colouring parts of the blood, it is then allowed to remain for a few days in a saturated solution of common salt, prepared with less care, and is then transferred to the vessel in which it is to remain. Evaporation is less apt to occur, than when

spirit is employed, and as crystalization would take place if this should happen, supposing the solution to be saturated, Mr. Cook reduces it a little, by adding half an ounce of very clear water to a quart of the saturated solution. Instead of quill or bristle, he uses delicate pieces of glass for separating and displaying the preparation, as they look neater, and are applied with more facility. In closing the vessel which is a very important part of the process, he first makes the rim of the glass perfectly dry, and then spreads on the rim of it some resin with a common spatula. A piece of glass which has been previously fitted, (and which should be a little smaller than the circumference of the rim, lest on lifting the bottles by the top, which is generally done, the glass might be raised,) is then applied, and by holding the warm spatula pretty close to it, the resin is softened, and the top fixed with the utmost accuracy. With small glasses, it is sufficient to make the resin smooth, and then varnish it over, but in bottles of larger size, additional security is obtained by attaching a slip of bladder round the edge by means of glue.

Mr C. affirms that this method is as easy as any other, and that 10 pence per gallon will pay all the expence, with the exception of the glass.

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### LITERARY NOTICES.

*Practical Illustrations of Typhus Fever, of the Common Continued Fever, and of Inflammatory Diseases, &c. &c.* By JOHN ARMSTRONG, M. D. Physician to the Fever Institution of London; with notes, Critical and explanatory, by NATHANIEL POTTER, M. D. Professor of the Theory and Practice of Medicine in the University of Maryland, &c. &c. First American from the third English Edition. 8vo. pp. 468. Philadelphia. James Webster. 1821.

Dr. Armstrong is one of the most popular writers of the present day.—The perspicuity and elegance of the language—the accuracy and variety of the observations, and the sound views, both practical and theoretical, which mark the productions of this author, place him among the very foremost of medical writers. The work on Typhus is certainly the ablest of his productions; and on which indeed the high reputation of the author seems mainly to rest. We know of no book, either ancient or modern, which contains more sound doctrine, concerning the nature and treatment of typhus. The notes added by Dr. Potter in this edition, are copious and possess considerable value.

*Flora of North America.* By WILLIAM P. C. BARTON, M. D.

We shall review this very splendid and meritorious work in our next number. In the mean time however, we must express our sincere wish, that an enterprise so laudable, so interesting, and so honourable to the author and to our country, may meet with that liberal encouragement, which it so well deserves.



## PREPARING FOR THE PRESS,

*An Essay on the Principles of Medical Education*, adapted to the genius of this country; and an Inquiry into the Causes tending to depreciate the American Medical Character. Addressed to the members of the medical profession, the trustees and directors of medical schools, and to students of medicine in the United States. By WILLIAM P. C. BARTON, M. D.

This work will be an attempt to show, that the physical resources of this country are adequate to the production of a National Pharmacopœia; that, consistently with the independent policy of its government, they ought to be laid under extensive contribution, to lessen our dependence, in so far as medicines are concerned, on foreign countries; that it would therefore be wise to incorporate such collateral branches of medical education, with the existing system of our schools, as shall be best calculated to promote a desire and ability to explore our native riches, and render them subsidiary to our independence. The work will also be an endeavour to prove, that such a plan, by fostering native genius and talent, will augment that *national scientific and literary reputation*, which sound and liberal elementary knowledge can alone ensure. It will also contain a free exposition of a defective system of private instruction, which, by a circumscription to *text-book information*, tends to detract from the dignity, and lessen the usefulness, of the profession of medicine, and to depreciate the character of any institution with which it may have acquired a virtual, though unauthorised connexion.

The author hopes to establish in the public mind, the disadvantages of those courses of elementary studies, which, by the unreal foundation they lay for future learning, may justify the Edinburgh reviewers, for years to come, in asking, "*what does the world yet owe to American physicians and surgeons?*"\*

## THACHER'S DISPENSATORY.

We have been requested to announce that the fourth edition of Thacher's Dispensatory, considerably improved, and so modified as to correspond with the National Pharmacopœia, is now in the press. It is our intention, in the next number, to give some account of this valuable work, and to compare it with another which has been written in this country, some singular circumstances relating to which, having come to our knowledge.

## UNIVERSITY OF MARYLAND.

An exchange of chairs has taken place betwixt the PROFESSORS of ANATOMY and SURGERY in this Institution. The Lectures on SURGERY will in future be delivered by JOHN B. DAVIDGE, M. D. and those on ANATOMY by GRANVILLE SHARP PATTISON, Esq.

## ERRATA.

In page 152, line 21 from the top, in our last number, for *external surface*, &c. read *internal surface*, &c.

Page 210, line 7, for *è senna*, read *ċ senna*.

Page 211, line 3 from the bottom, for *é decoct*, read *ċ decoct*.

\* Edinburgh Review, No. LXV.

**SUPPLEMENT**  
**TO THE**  
**AMERICAN MEDICAL RECORDER.**

VOL. IV.—NO. II.

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APRIL, 1821.

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TO THE EDITORS OF THE MEDICAL RECORDER.

GENTLEMEN,

I HAVE to acknowledge your attempt to rescue the "humble merit" which is justly due to me, for the invention of the Portable Hot or Vapour Bath; which, it appears, Dr. Johnson of Great Britain, "in his Medico-Chirurgical Journal, has bestowed upon Charles Gower, M. D. Fellow of the Royal College of Physicians, and Physician to the Middlesex Hospital, with compliments to the modesty and ingenuity of the contriver." And, as I was contemplating a correction of the mistake, through some respectable periodical publication, you have kindly anticipated my wishes, and saved me that trouble.

But, in doing me this favour, you have imposed upon me the necessity of troubling you and your readers with my apology for having adopted an expedient in regard to this invention, which, without explanation, may appear to have merited animadversion; if my explanation be insufficient, I am ready to receive the reproof with grateful submission. For the sentiment is substantially correct, that it is not "decorous in a member of a liberal profession, either to conceal or obtain patents, for any inventions or discoveries which he may have made, tending, as he supposes, to diminish the mass of human suffering," if it be done from mercenary motives. A plain and honest narrative of the facts and circumstances which shaped my conduct, it is hoped, will exculpate me from merited censure, and satisfy your candid readers, that, however far I may have erred, my intention was honourable, and the course which was taken the best, in my judgment, for the accomplishment of the only proper object, the speedy "diminution of the mass of human suffering."

Fifteen years ago, after much careful observation, I formed most of the opinions which I now entertain relative to the importance of the functions of the skin, in the preservation of a healthful condition of the human body; of the indications it furnishes in aid of a judicious pathology; and of the certain and extensive advantages to be derived from appropriate applications made to it in the practice of medicine. And I am much gratified to find, that the distinguished Mr. Charles Bell supports me in them all, to the fullest extent. "The more important function of the surface," says Mr. Bell, "is to be contemplated in its effect on the general activity of the vascular system, and in the vicarious action which takes place betwixt it, the stomach and intestines, and the kidney and lungs. The similarity of function performed in the lungs and by the skin, would lead us to attend to the injury of the former, by the impression of cold on the surface, and the checked perspiration. The fact, that



“perspiration is altered in degree by the progress of digestion, would lead us to attend to the many occasions in which we see the disorders of the viscera affecting changes on the skin; the imperfection of the function of perspiration, when digestion and the function of the viscera are deranged, would lead us, not only to mark the symptoms of internal disease, on the skin, but to take the means of exciting the latter as a remedy for the former. In the same manner, will the secretion of the kidney be influenced by the state of the skin and of perspiration: need I add, that the health and strength of the circulation, and of course the health of all the functions, is influenced by the excitement of the skin. Some practitioners take the stomach, and others the bowels, and others the liver, on which they harp continually;—let any one take the skin as his object of cure; and his practice will have equal success, his cases and facts become soon as numerous, while his connection with general science will be more intimate; and, if he introduce his system by shewing that health is enjoyed when the various functions, which together form the animal economy, are perfect, and that one function cannot be in health without the whole be also, he will, in my opinion, have better claims to public favour, than any who have yet flourished in it by promulgating doctrines in regard to the functions and diseases of individual parts.”

Of the various agents which were employed as excitants to the skin, in conducting the experiments and observations, by which I was confirmed in these opinions, none was found so speedy, general and effectual, as heat. Many expedients were therefore tried, with a hope of obtaining a mode of applying this agent in the most perfect and agreeable mode. From time to time different devices were used; and after comparing their results, none was found so effectual as the steams of boiling water. This method however, although powerful, was subject to many objections. If sufficiently hot to produce a decisive effect, it approximated too nearly to a scald; and if sufficiently cool to shun this inconvenience, it would often serve to chill rather than to heat the patient. Besides, the moisture which it necessarily imparted to the linen and bed-clothing of the patient often presented an insurmountable difficulty. At length I had recourse to ignited alcohol, and invented a portable apparatus for the purpose of directing the heat. By this contrivance, a large volume of heated air, without an excess of humidity, was prepared and conducted, in a way sufficiently convenient, for general or topical application; and I was enabled to multiply my experiments to any necessary extent.

After hundreds of repetitions, I was convinced, that heat applied decisively in this dry way, was almost invariably effectual in correcting predisposition to febrile disease; that such disease, when actually formed, if recent, aided by a little depletion, when necessary, might be relieved by the same method, in a manner unusually pleasant and speedy;—and, that the treatment thus far, might be safely conducted by good sense, without much pretension to any part of medical skill, except only the necessary decision. I had the happiness to learn, moreover, that, in the hands of the scientific and skilful physician, it would place within the reach of the healing art, various other forms of disease, which had stood as lasting monuments of reproach to the profession. The convenience and value of the portable tube became of course daily more conspicuous, and I could not do otherwise than consider it valuable; indeed, such was my conviction of its worth, that I was confident it would be highly appreciated by every enlightened physician. In this expectation I was the more sanguine, as several respectable physicians of Lynchburg and Richmond, Virginia, after having examined the apparatus, and observed its application and effects, approved of it and furnished me with certificates to this purpose.

It was therefore thought advisable, to prepare and publish a pamphlet presenting a brief and comprehensive view of the subject, and with this auxiliary, to make my advances to gentlemen of the profession, as subsequent circum-

stances should direct my course. The pamphlet was published, and copies were distributed with solicitude and care. One which was sent to New York, afforded the writer of the Review in the Medical Repository, vol. ii. New Series, No 4, a pleasant opportunity "to shew his utter repugnance to the medical and moral unities which the author had adopted." But the publication produced no important result in the metropolis of Virginia.\*

It received a respectful attention in the city of Washington, and I was invited to the seat of government. After much conversation, continued at intervals for several days with Gen. Tilton, and after having answered many important and detailed inquiries made by the secretary of war, respecting the theory and practice which I had submitted, it was determined, that I should visit the military station at Norfolk, and introduce my system into the hospitals at that place. In conformity to the orders of Gen. Tilton I was accompanied by a hospital surgeon, who was instructed to assist me. In about three weeks I prescribed to more than 90 patients; out of which the surgeon accompanying me saw about 30 under treatment—a sufficient number to satisfy himself of the propriety of making a favourable report; in consequence of which, the remedy was partially and imperfectly employed, at different stations of the army.

It was the wish of the secretary of war and of Gen. Tilton, that I should have accepted the appointment of hospital surgeon, and taken the charge of the Norfolk station. This would have been the more prudent course; I was so confident, however, that the notice taken of me by government, would answer every necessary purpose, that I refused any official appointment, and solicited the arrangement which was made in conformity to my request. I was led into this error the more easily, because, I had no wish to remain in the army. I chose rather to hasten to the seats of medical science, submit my contrivance with my views of its utility to men at the head of the profession, determined, if it met their approbation, immediately to give an unlimited privilege to every regular physician in the United States, to employ the remedy at their discretion. The great readiness with which I had been received at the seat of government, made me quite certain in my expectation, that the professors of the Universities of Pennsylvania, New-York and Maryland, would enable me at once to execute that purpose.

Full of complacence from this fond expectation, I obtained from the president of the United States, letters to Dr. Physick, and to Dr. Samuel L. Mitchell, and from the Secretary of War to Gen. Bloomfield and the Commissary General at Philadelphia, and to other gentlemen of weight and influence in New-York, confident, that a contrivance, which needed no more than a moment's inspection to prove its convenience, and but few applications to prove its power, when presented under convoy of such dignified patronage, would meet expected approbation.

*Washington City, August 1814.*

Dear Sir,

Doctor Jennings has a medical invention, in the value of which he feels so much confidence, that he is anxious to present it to the consideration of the most enlightened of the profession. Although a departure in some measure from an established rule, I cannot refuse a line which may promote an opportunity for the explanations, by which he wishes his invention to be tested. His benevolent character, is a further apology for the liberty I take.

Accept assurances of my great esteem and friendly respect.

JAMES MADISON.

*To Doctor Physick.*

\* Letters patent were not obtained until I met with these discouragements in Richmond.



The President's letter procured me an interview with Dr. Physick, who seemed to be pleased with the contrivance, and obligingly accepted a system of the bathing apparatus. He was in ill health, however, and I saw him no more. Indeed, I know not, whether he ever used it on any occasion, having had no communication from him upon the subject.

From Dr. Barton much was expected. But unfortunately, the pamphlet which I had published, exposed my attachment to the general principles taught by Dr. Rush. And as it was my principal intention, to make known my experience in the use of the bath as a general remedy, it was quite natural, in an interview with the professor of the theory and practice of physic, that I should have dwelt upon that point, which I conceived to be most emphatically entitled to the claim of useful and important discovery.\* The professor in his reply, made severe animadversions upon the late innovations of Dr. Rush;—informed me, that a laudable attempt was then making in the University, to correct the growing evil; that, he in particular was engaged in editing a new impression of Cullen's First Lines, with notes by himself, and obligingly submitted to my inspection, several proof sheets sent in that morning for correction. He seemed to have no doubt of extirpating the baneful doctrines, and therefore signified, that it would be prudent on my part, to abandon the intention of associating any remedy with principles so erroneous, and which of course would so soon become obsolete. And in short, if I persevered in my purpose of maintaining such opinions, "*that single circumstance would damn the invention.*" He afterwards attended with the physicians of the hospital, and made observation upon one application only, of the bath, and I heard from him no more.

Through the influence of Gen. Irvine, I had an introduction to Dr. Wistar. The politeness of this very distinguished Professor made me acquainted with the physicians of the Pennsylvania Hospital. After a medical conversation with these gentlemen, in which Dr. Wistar and others of the faculty took a part, access was given me to the Hospital; and I was permitted to select whatever cases I thought the best fitted, for a display of the operation and power of the remedy in question.

This done, my mind was at rest. Although I was deprived of the aid which I had expected from Dr. Physick; and even although Dr. Barton had threatened destruction to my labours, nevertheless, as an opportunity was now given me to demonstrate my practice under the observation of medical philosophers, I was confident I had resources in the candor and vast attainments of these great men, which could not fail me. With much zeal, therefore, I proceeded to administer to my cases. But, just as I had began to give evidence, in my judgment the most unequivocal, that the remedy would afford extraordinary advantage to several of the patients; at the moment when I expected congratulations from the attending physicians, I was stunned by a communication which informed me, that Dr. Otto, in his clinical remarks had stated in the presence of fifty or more young gentlemen, "that the new remedy had been employed in the cases under observation, that although in some of them, there were considerable changes; still, as other powerful agents were likewise employed, as auxiliaries to the bath, it would be difficult, if not impossible, to determine whether any or how much value, was to be ascribed to its use." I found that Dr. Wistar concurred with him in the same opinion, and defended its propriety; I was compelled therefore to admit, that I had been exceedingly unfortunate, since I had in hundreds of instances placed an undue estimate upon this favourite agent; or if I had not committed this monstrous mistake, great men needed instruction to prepare them to make the discriminations, which if

\* I have great cause for believing that upon this point I am not yet understood. If I were, no physician knowing as I do the power of heat would be without the bath.

made, would justify my opinion in its worth. There remained but one alternative. The patients which had been committed to my charge were resigned, and I considered myself politely dismissed from the Hospital.

I had received considerable civilities from Dr. Parrish and Dr. Hewson, at that time attending physicians to the Alms-house. These gentlemen permitted me to use the Bath, as an auxiliary remedy to their practice in several cases; and from them I received the following certificate.

*Philadelphia, 8th mo. (August) 20th, 1814.*

"In our attendance in the Alms-house of this city, we have had several opportunities of witnessing the use under the direction of Dr. Jennings, of the warm Bath patented by him. It appears an economical and expeditious mode of applying heat, and in the instances which fell under our notice, was uniformly followed by a plentiful diaphoresis."

"JOS. PARRISH

"THOS. T. HEWSON."

Having obtained this paper, it occurred to me, that some additional worth would be given it, if it were accompanied by a history of the cases in which they "had witnessed its use." Application was therefore made to the young gentleman who kept the diary of the Alms-house practice, for such extracts from his book, as might answer for that purpose. And after considerable delay and hesitation a few fragments of cases were furnished me.

The first case as noted in the extract, was stated as follows, viz. "Lydia Black, aged 46 years, was admitted into the Alms-house, July 11th 1814. She had had a troublesome cough, upward of three years, and lately symptoms of hydrothorax.

"August 11th. Was taking *Scillæ grs. ii. Cal. grs. i. opii. Grss. ter. die.* When the Alcoholic Steam Bath was directed. At 6 o'clock P. M. the Bath was used. Her pulse at this time was so small as to render it very difficult to ascertain the number of strokes; supposed however to be 110.

"She felt easier for an hour after the operation. Her pain and difficulty of breathing somewhat removed, but she shortly after returned to her former feelings." And here ended the extract.

I requested a continuance of the history to the close of my practice. He insisted, he had given me all that was necessary or proper in the cases; and finding him inflexible, I made my appeal to Dr. Parrish, who directed him to proceed. He then upon a separate scrap of paper, gave the following addition, viz. "Two days afterwards, the Alcoholic Steam Bath was repeated. The pulse was increased in frequency and strength. The patient, on the following day, was in a condition to receive mercury, which was used in the form of Camphorated Mercurial ointment, by friction: since which time, she is salivated and continues to improve."

The case of Lydia Black presents a fair specimen of the manner in which I associated other remedies with the use of the bath. The history which was taken, though true as far as it goes, was very incomplete. The patient was taking squill. Cal. and opium. So far however, was the prescription from answering any valuable purpose, she was sinking under its use. She laboured under dyspnœa the most distressing imaginable, her skin cold and clammy, at the same time that her pulse was almost imperceptible. Every physician who has had experience knows what must have been the situation and prospect of such a patient. In requesting the privilege of treating her case, I stated my expectation, that she would revive upon a repetition of the genial influence of heat; and that her sinking system would possibly be prepared to acknowledge the agency of mercury—and if she should not recover she would at least afford a fine opportunity to test the power of the Bath. The result sufficiently proved the correctness of my prognosis. This fact however, with a hundred others of similar import, had they occurred, could



have answered me no valuable purpose. For we had associated with the use of heat, an auxiliary and powerful remedy, "Camphorated mercurial ointment by friction." And therefore it was "extremely difficult if not impossible to determine, whether any or how much value was attributable to the bath."

The notices of the four cases which are subjoined, added to that of Lydia Black, will show that the bath did indeed produce a perspiration and something more;—imperfect and unfinished as these notices are.

"Margaret Gardiner, aged 40 years, admitted with dysentery.

"August 9th. Skin dry, pulse 84. 20 stools in 24 hours; the steam bath was applied, and after the operation her pulse was 94. Seven hours after the operation, her pulse was the same as before. She has been taking, and continues to take, the Mist. Oleaginosa.

"August 10th. Patient better. Number of stools somewhat diminished. Skin moist. Other circumstances the same. Repeat the steam bath. 6 o'clock, P. M. before using the diaphoretic apparatus, her pulse was 112. She complained of pain in the back of the neck, in the ankles, and in the bowels. In 15 minutes her pulse was 124. In 30 minutes, the thermometer under the bed-clothes, rose to 90°. In 40 minutes, pulse 131, thermometer 110; sweating profusely. In about 45 minutes the apparatus was withdrawn.

"The patient felt more comfortable. The pain in the back and ankles easier. Pain in the bowels not much diminished. Two hours after, pulse 104.

"August 11th. Patient rested better. Stools not so frequent; small, and attended with less griping and tenesmus. Skin moist. Pulse 100."

I thought the bath entitled to acknowledged merit in this case. The history of two days only is given. It might have been added; the patient was recovered within half the period usually required in similar cases.

"Susan Winn, aged 19 years, intermittent fever. About 8 days since, was attacked suddenly with a chill, which was succeeded by a fever, and terminated by a sweat.

"The chill occurred about 1 o'clock in the afternoon, and has returned every day at the same hour; progressing and terminating in the same manner.

"August 18th. At half past 11 she was sweated, pulse 85 before the operation. At 12 o'clock pulse 106. At 1 o'clock, 88. The operation lasted 40 minutes, and a copious sweat was produced.

"Aug. 19th. Complains of a slight degree of nausea. Bowels not moved since yesterday. Tongue clean. Pulse 70. The chill did not recur yesterday.

"Aug. 20th. The chill did not recur yesterday."

Here, one application without other powerful remedies, cured a regular intermittent, of eight days standing. I thought this worthy of notice.

"Peter Vanest, aged 59 years, has had a rheumatic affection for some time past; for which he has been treated. An apparatus for promoting sweating was used for this purpose, commencing at 11 o'clock, A. M. August 8th. Pulse at this time 62; limbs stiff; some degree of pain; 12 o'clock, considerable diaphoresis, pulse 64; pain easier; stiffness somewhat diminished.

"James M'Instry, aged 36 years, complains of difficulty of breathing somewhat resembling asthma. Has pain in the side, pains in his knees and feet; skin very cold; has been taking Dover's powders; pulse 100; at half past 11, used the steam apparatus; 18 minutes before 12, pulse 112; considerable diaphoresis; 10 minutes before 12, pulse 120.

"The difficulty of breathing somewhat diminished. Pain in the side much relieved, but little diminution of pain in the ankles."

Considering each of these to be the result of a single application of the bath,—considering each to have been a chronic and obstinately unmanageable case, I thought no article of the Materia Medica known could in thirty or forty minutes have done more.

These five were a part only, though perhaps the most important of the in

stances in which the bath was used in the Alms-House. And to say the least, the certificate was a very modest one, and the cases sufficiently abridged.

A greater number of cases, some of which as fully evinced the alterative power of heat in chronic and unmanageable diseases, were exhibited in the hospital. Besides these, a patient of Drs. Duffil and Griffiths, a very large and fleshy man, his name not recollected, was labouring under enteritis with incorrigible constipation. When I was called in, had been treated without effect for a whole week and was ready to sink; his pulse almost imperceptible, his skin cold and clammy, and his physicians expecting a fatal termination. Indeed they did not hesitate to admit, that he presented a proper case for making the experiment, as his disease defied all *ordinary remedies*. In this instance, if my memory serve me, Dr. Chapman, Dr. Caldwell, the two physicians above named and one or two others, had made themselves acquainted with the dangers of the case. After two repetitions of the bath, cathartics became effectual and the patient was recovered. But in all these instances excepting that of Susan Winn other powerful means had also been used; "it was therefore difficult if not impossible to determine, whether any or how much value should be ascribed to the bath."

Perhaps I was wrong, but it was my opinion at the time, that I had been treated illiberally at the Hospital, and that Drs. Hewson and Parrish would have given me a more satisfactory certificate, if it could have been done without contradicting the opinions of gentlemen highly worthy of their esteem.

After such a complete disappointment what remained to be done? I had made incessant attempts, in ways which presented themselves to my apprehension as being the most proper, to place the contrivance in the hands of the faculty. And increased exertions served only to render the prospects of success, more and more gloomy. In other places many physicians openly condemned the bath as pre-eminently dangerous. Indeed at that time, a few individuals only, seemed to approve it, under any circumstances. It had become my duty to make its simplicity, excellence, power and safety, known to the community. If the professors and other distinguished physicians in Philadelphia had been convinced of the propriety of acknowledging the truth of my views, my labours would have been very soon brought to a close. Under the influence of their high authority, almost every other practiser of medicine would have received the contrivance with gratitude, and a knowledge of its value would have been vastly extended. It appeared to me, that I was driven to the necessity of depending chiefly upon the effects which I knew would be produced in time, by the use of the bath, as a family article. I had too much cause to fear, that I should be compelled to this measure, when the patent was at first obtained. Two reasons then urged the propriety of taking out the patent.

First; Physicians required proof of its worth, before they could give it their sanction, and so far as I was informed, when the patent was obtained, they were not willing to prove it for themselves. I knew that common sense, with a few simple instructions would be able to use it, with satisfactory success; without the aid of medical science or medical men. I knew that its use was accompanied with less danger than that of the lancet or of calomel and jalap, which were in the hands of almost every planter and overseer in our state. It was therefore, a very safe conclusion, that I should one day, by this plan, be able to give the proof which the Faculty required. But to keep the remedy in the hands of good sense, it was necessary that it should be vended under the restrictions of a patent, and that its price should be respectable. And I believe that even an enemy will admit, that ten dollars was a price sufficiently high, to produce that effect.

In the second place I knew that whenever I should find it proper, to take off the restrictions of my patent in favour of medical men, the simplicity of the practice is so great, that every nurse accustomed to give a sweat in whatever ordinary form, would certainly soon give my method the preference.



Merely to produce a perspiration with certainty at the time I required it, was a very small part only of what I had learned from the use of the bath. I had ascertained, that heat would become one of the most valuable medicaments; and I wished to restrain its use, in such a manner, that whosoever should employ my contrivance, should be induced to read a pamphlet calculated to make its value known not as a mere sweating machine, but as an agent powerful in the cure of fever. And an intelligent reader of the pamphlet, which accompanied the bath, must necessarily perceive, that it was not intended for the vulgar eye. To this view especially, as the only very important consideration, in my opinion of the subject, I called the attention of General Tilton and of the secretary of War. And to this I referred in my application to the president of the United States for his letter to Dr. Physick. My candor nor my benevolence in the affair, was suspected by these disinterested great men. They and I, expected my success. But my failure led me to think myself suspected by the physicians, of an intention to take advantage of their approbation for the purpose of multiplying my sales, of course, of introducing a family article, calculated to interfere with the interests of the profession, and therefore that very few could feel the worth of my hints.

I have since reflected upon the subject, and have now very little inclination to impugn their motives. Time has well nigh brought about all that I ought to have expected. And although the pains which I then took to conciliate the friendly feelings of the profession, seemed ineffectual and were therefore calculated to produce mutual disrespect and distance, it is my present conviction, that a degree of hesitation, the just expression of an honest mind, feeling the want of additional evidence, was but too readily misconstrued by me to be the effect of a disposition inimical to my system. I should not have overlooked the fact, that the prosecution of the subject, for the space of ten years, together with many hundred opportunities of seeing the effect of heat internally and extensively to the surface, had at length made propositions irresistibly clear to my apprehension, which upon the statement of an individual supported by a few facts only, might have appeared problematical to other men, of very superior abilities.

Some time after my retirement from Philadelphia this very just view was taken, and I began to prepare a volume, intended to lay before the public, the whole matter in extenso. This work is still in a state of progress; and would have been ready for publication before this time, but a long and severe affliction of my family, and other unavoidable circumstances have hitherto hindered its completion. When this undertaking was commenced, I had determined, so soon as it should be issued, to take off the restrictions of my patent, in favour of the faculty agreeably to my first intention, and communicated that resolution to Professors Potter and McDowell, and other gentlemen of my more intimate acquaintance. And it has been deferred for no other reason, than that the explanatory work is not yet finished. The respectable notice taken of my former attempts by Dr. Thacher in his "Modern Practice," by Dr. Mann in his "Medical Sketches," and by Dr. Gallup of Vermont in his treatise upon Epidemic Diseases, and above all the very polite and friendly treatment which I have received from the professors of the University of Maryland and from others of the Faculty of this state have served to remove the erroneous impressions which had been made upon my mind as to the disposition of physicians, and have elicited a wish for an appropriate opportunity to make known my acknowledgements. Here I ought to add, that many individuals in other States, have, from time to time, given me their approbation; and Dr. Samuel L. Mitchill of New-York, at an early period, a little more than one year after my visit to Philadelphia, favoured me with a second letter, which I beg the liberty to subjoin.

*New-York, 29th Oct. 1815.*

DR. SAMUEL K. JENNINGS,

Dear Sir—Since I wrote to you and General Tilton about the Vapour Bath I have had many opportunities of trying its practical efficacy. I am more and more satisfied with the value of the remedy. It is so neat, so cheap, so handy, and so within the reach of every body, that it surpasses every thing with which I am acquainted for applying caloric to the external surface of the human body. It is capable of conveying heat, so susceptible of a higher or lower temperature, and withal so well adapted to a longer or shorter continuance, that its power to be beneficial is exceedingly enlarged.

But above all, the caloric may be directed to a part of the body, or to the whole body, to a robust person or to one in the lowest debility, to an adult or to an infant, with so much ease and benefit, that I really consider it one of the most happy expedients that a practitioner can employ.

For chills, torpors, and that apathy and languor of the cuticular surface, so often the forerunner or companion of fevers, it is preferable to any other mode of applying heat. The warm bath, warm fomentations, and the vapours of hot water are very inferior to it.

The patients in our hospital are exceedingly pleased with it, and for myself, I explain it, I order it, and I recommend it.\*

Accept the assurances of my high esteem, and regard.

SAMUEL L. MITCHILL.

It would have been a source of gratification to me, if the work which I have on hand, could have been finished when this publication should have been made. But as it is not probable, that it can be accomplished very soon, and as the animadversions in the last number of the Medical Recorder seem to have made this a fit occasion for executing a long contemplated purpose, I hereby make it known, *that from and after the first day of January 1821, all regular physicians throughout the United States, are at liberty to make use of the "Patent Portable Warm and Hot Bath," whenever and wheresoever they may think proper, without restraint or limitation. And as the only additional apology which I think it necessary to offer for my having obtained, or for retaining my patent privileges in respect to the contrivance as an article of family convenience, I will be believed by those who know me, when I declare, that I have not realized one cent by any profits yet made upon it. It was intended to employ the whole amount in a benevolent way, and more than has ever yet been received, has been expended in endeavours to extend its use, from Maine to Orleans.*

After my contemplated publication shall have been made, the bath, in a form the most simple, durable, and convenient, will probably be thrown into the market, at a low price as an article of commerce.

I am now engaged in some additional experiments, intended to reduce the tube into a form and compass, the most suitable for the use of physicians. The results will be published, with such descriptions, that every practiser of Medicine who may choose to procure it, may have it made by any copper-smith, in his own neighbourhood. And I request, that this notice may be published, in every periodical work, in the United States.

SAMUEL K. JENNINGS.

*Baltimore, 16th Nov. 1820.*

\* See also the Med. Repository, Vol. iii. New Series, No. 2. page 159.



## ANALECTA.

*Of the Radix Ratanhiæ.*

DR. RATH, of Nordhausen, has published some remarkable cases of passive Menorrhagia and fluor albus, which, after resisting all the usually employed remedies, as cinchona; acid sulph; dic. catechu; ext. lig. camp; alumen. ipecac. opium. hyoscyamus, &c. &c. as well as injections of oak bark, galls, &c. were very easily cured, by taking for a short time, a decoction of this root, in the proportions of one ounce of the root to eight ounces of fluid. There are three cases of menorrhagia, three of fluor albus, and one of emaciation and debility, from an almost constant seminal discharge of a young man, who had been addicted to onanism and sexual connexion at an early age. In each of these, we are told that all the hitherto known resources of medicine had failed, and, of course, that the Rad. Ratanhiæ alone saved the victims from a premature grave. If this be not exaggerated, it is, indeed, a valuable addition to our materia medica, although Dr. Rath tells us it failed in two cases of consumption, to remove the colliquative sweats and diarrhœa, &c. "because, perhaps, the disease was too far advanced, and the powers of life too much depressed to be assisted by even the best means."—*Hufeland's Journal der praktischen Heilkunde*, Junius, 1819.

*Vomiting successfully induced by Transfusion of an Emetic into a Vein.*

A middle aged man swallowed a large bone, which stuck fast low down in the œsophagus, from having assumed a transverse direction; and notwithstanding innumerable attempts were made by different surgeons during three days, both to extract it and push it down into the stomach, it remained fixed in its place. At the end of the time mentioned, the man came into the hospital, his strength greatly exhausted, his breathing laborious, the face and extremities livid and cold, and the bone continued immovable, as the patient was unable to swallow a single drop, which rendered the exhibition of an emetic in the usual way impossible; an incision into the œsophagus was impracticable from the low situation of the foreign body, and as the symptoms became every moment more urgent, it was determined to inject a solution of tartar emetic into a vein. For this purpose, two grains of that substance were dissolved in half an ounce of lukewarm water, and the whole injected into the median vein. At the expiration of fifteen minutes, which the patient passed under some feelings of anxiety, severe retching and free vomiting suddenly took place, during which the bone was ejected quickly and forcibly from the mouth. With this the whole of the patient's distressing symptoms ceased, and the slight contraction which remained at the place where the bone had stuck, shortly disappeared.—*Bericht über das klinische, chirurgisch-äugenaerztliche Institut des Universitaet zu Berlin, abgestattet vom Director des genannten Anstalt Geh. Rath Graeffe*, 1819.

*Account of the Effects produced by injecting an Aqueous Infusion of Fox-glove (Digitalis purpurea) into the Jugular Vein of a Dog.* Communicated by RICHARD PEARSON, M. D.

IN the following experiment the operation was performed by Mr. Jukes, house-surgeon to the Birmingham Hospital, in the presence of Dr. Booth, (one of the physicians to that institution,) and of myself. Immediately after the operation, Mr. Jukes undertook to note down the result, which I transmit in his own words.

"April 20th, 1820.—I exposed the external jugular vein of a small sized terrier, and injected two drachms (by measure) of the infusum digitalis, (of a

blood-warm temperature,) containing  $7\frac{1}{2}$  grains of the dried leaf. Before I could sew up the wound, the dog was seized with violent struggling, which lasted for a minute or two, followed by a drowsy state, though without sleep.\* I make no doubt he would have remained several minutes in this condition, had we not have placed him on his legs. His pulse previous to removing him was 64 in the minute, (being about 7 minutes after the injection,) and evidently affected by the foxglove. In 2 minutes more, he voided a costive stool. Between the one-fourth of an hour and 20 minutes, his countenance depicted considerable anxiety, and he frequently elevated his head, putting his throat on the stretch. At the expiration of 20 minutes, saliva flowed involuntarily from his mouth. Pulse exceedingly small and intermittent, rarely exceeding 58 or 60 beats in the minute. At 22 minutes, he vomited, and in the effort caused a slight hæmorrhage from the wound. At 25 minutes he voided a small quantity of urine, seemed distressed, and began to run about. Pupils at this time had become widely dilated, but possessing the power of contraction on exposure to vivid light. No paralysis whatever.† At 35 minutes pulse even weaker, 60 beats in a minute; apparently more drowsy, or at least a frequent closing of the eyelids, but not insensible to noise. At 40 minutes, after exhibiting a sense of dryness of the fauces by motions of his mouth, he eagerly lapped water which was offered him. At 47 minutes he again lapped water, immediately after which he got up, stretched himself, and vomited up the whole. He then crept into a corner, and curled himself up for sleep. For several hours after he continued occasionally sick and lazy. The next morning he was quite recovered."

I cannot help availing myself of the present opportunity to remark, that no physiological experiments were ever so badly conducted as those by Courten, related in the 27th vol. of the *Phil. Trans.* Fifty grains of opium in an ounce of water, injected at once! One ounce of camphorated alcohol, in another instance, injected at once! One ounce of decoction of tobacco, used in like manner at once! Death, as might be expected, was in every one of these cases the result. But if only the 25th part of the opium solution, the 30th part of the camphorated alcohol, and the 40th part of the tobacco decoction, had been injected, the animals (it may be presumed) would not have died, and the facts would have been interesting.

P. S.—In my former letter, I mentioned the circumstance of several persons in this neighbourhood having been last summer bitten by a mad dog. Excision of the bitten parts was resorted to at the time, (in one case not till three days after the bite,) and all have hitherto remained free from the disease.—*Edin. Med. and Surg. Journal*, January, 1821.

*Test for Barytes and Strontia.*

Barytes and strontia may be readily distinguished from each other by the following process: Make a solution of the earth, whichever it may be, either by nitric, muriatic, or some other acid, which will form a soluble salt with it: add solution of sulphate of soda in excess, filter, and then test the clear fluid by subcarbonate of potash. If any precipitate falls down, the earth was strontia; if the fluid remains clear, it was barytes.—(*Journal of Science and the Arts*, vol. x. p. 189.)

\* That is to say, a dull and quiescent state,—a state of collapse, but without actual sleep.

† Some days previously, 4 grs. of opium diffused in 2 drachms of distilled water, had been injected into the jugular vein of a different dog, producing in the first instance coma and stertor, and afterwards paralysis of the hind limbs. The animal was well the next day.



*Vegetable Antidotes to Poison.*

Dr. Chisholm, in a paper read to the Society at Geneva, states that the juice of the sugar-cane is the best antidote known for arsenic. It has been tried upon various animals in the West Indies with complete success, and always succeeds. Its power in the island of Nevis is generally known.

Dr. Chisholm also mentions the singular powers of a plant, well known to the Indians, as a remedy for the ophthalmia; it is called *akouserounie* and *warrannie* by them, and *eye-root* by the white people. It grows in *la Guyane*, in the neighbourhood of Demerara, in a sandy soil, and is a species of bignonia which Dr. C. has since called *ophthalmica*. An Indian prepares the remedy from the root of the plants, by first stripping off the brown epidermis, and then separating a fibrous pulpy part immediately beneath; this he presses on cotton so as to collect the juice, and then by means of a paper funnel conveys a drop or two of it into the eye. This is repeated once a day for three or four days, in which time the cure is generally completed. Dr. Chisholm had occasion in his own practice to apply this remedy in three cases; and having only the dry root, he rased off the outside, and then made a strong infusion of the part beneath. Six drops of this infusion were introduced into each eye once a day, and in six days treatment they were perfectly cured, though they had suffered for many weeks previously.—*London Med. Repository*, December 1820.

It has been always known, both to Physicians and Surgeons, that fomentations, or the application of moisture at a certain temperature, had great efficacy in allaying pain and abating inflammation; but hitherto no means have been devised for the continued employment of this remedy. An apparatus for this purpose has been lately contrived by Dr. Macartney, of Dublin, which promises to produce the most beneficial effects, even in recent wounds of the worst kind. In a case of punctured and lacerated wounds, in which the palmar fascia was penetrated and partially torn out, the hand was placed in the vapour of water at 97 degrees, Fah. immediately on receiving the injury, and detained in it during twelve hours, without intermission. By these means all pain and inflammation were prevented. Lint, wet with water, was subsequently applied; the wounds speedily healed without suppuration; the surface gradually closed, instead of filling up with granulations, and the cicatrices left are of the most possible kind.—*London Med. Repository*, January, 1821.

Professor Brera states in his Journal, (*Nuovi Commentaria di Medicini e di Chirurgia*, No. 2, 1820,) that Dr. Salvatori writes to him from Petersburg, to say that he has employed the leaves and flowers of the *campanula graminifolia* in epilepsy, with the most fortunate results, in many cases. The following is the formula in which he has ordinarily employed it:

℞. *Herb. et flor. campanul. graminifol. unc. semis*: infunde per horam in aquæ feridæ lib. una; deinde coletur; et sumat vasculum ter de die, scilicet, mane ante prandium, et vespere ante somnum.

ROYAL SOCIETY of London.—Nov. 9. A paper by Sir E. Home, was read, entitled “*On the Black Rete Mucosum of the Negro, being a Defence against the Scorching Effects of the Sun’s Rays.*” The author began by stating some observations which had induced him to form the opinion that the scorching effects of the sun’s rays are produced, not merely in virtue of their heating power, but by the joint agency of their heat and light. To verify this opinion, he made several experiments which showed that the face and hands may be exposed to a temperature of 100°, or even 120°, without pain being produced, provided the light be excluded: but that if the same, or even an inferior degree, of heat be produced by the direct rays of the sun, the parts are scorched, and blisters are produced. This effect he found to be completely

prevented by covering the hand or face with black kersemere; and the same purpose is attained by the black rete mucosum of the negro. In those cases when a black covering was superimposed, perspiration came on; and the same takes place on the skin of the negro, when exposed to the direct rays of the sun.

The author observed, also, that the eyes of those animals exposed to the strong light of the sun are furnished with a black pigment, apparently for the same purpose; while others, which are abroad by night, and consequently not needing such a protection, are not provided with it.—*London Med. and Phys. Journal*, January, 1821.

### THE PHILADELPHIA MEDICAL SOCIETY.

#### OFFICERS OF 1821-22.

*President*—WM. P. DEWEES, M. D.

*Vice Presidents*—{ JOSEPH PARRISH, M. D.  
JOHN BARNES, M. D.

*Orator*—N. CHAPMAN, M. D.

*Corresponding Secretaries*—{ JOHN EBERLE, M. D.  
SAMUEL JACKSON, M. D.

*Treasurer*—SAMUEL STEWART, M. D.

*Librarian*—GEORGE TRESSE, M. D.

*Curators*—{ GEORGE TRESSE, M. D.  
SAMUEL M. FOX.

The following gentlemen were elected honorary members.

Dr. John Stitt,	Gideon Humphrey, M. D. Pa.
Thos. D. Mitchell, M. D. Pa.	Gilbert Flagler, M. D. do.
Sir James Wylie, M. D. Russia.	James Cornick, M. D. Surgeon U. S. N.
James Leighton, M. D. do.	Dr. — Creighton, Russia.
Ellis C. Harlan, M. D. Pa.	

The following Students of Medicine were admitted as junior members:

*Maine*—John Hubbard.

*New York*—Seth Salisbury.

*New Jersey.*

John Elkinton,  
Henry Holcombe,

John Brick,

Richard Dobbins.

*Pennsylvania.*

Benjamin Ellis,  
Theophilus Dunn,  
Gust. Colhoun,  
Nicholas B. Lane,  
Wilson Jewell,  
Benjamin Sandford, jr.  
F. J. Le Moyne,

Wm. M. Fahnestock,  
Francis Waln,  
J. Rodman Paul,  
Benjamin S. Budd,  
John Purves,  
Jacob Jeanes,

Wm. Davis,  
Matthew Pryor,  
B. Rush Rhees,  
Chas. Markley,  
Eliphalet Stevens,  
Wm. Milnor.

*Delaware*—Bolitha Laws.

*Maryland.*

Wm. Plater,

Wm. P. Williams.



*Virginia.*

Wm. W. Wright,  
J. C. Taliaferro,  
Wm. A. Lacy,  
Geo. W. Perkenon,  
Thos. H. Averett,

Edmund Berkley,  
Christopher Terrell,  
Southey Satchell,  
George Morton,

John Ker,  
Theodoric A. Carter,  
G. Marion Lewis,  
James Cooke.

*North Carolina.*

Josiah C. Skinner,  
Smith Murphy,  
John Jackson,

M. W. B. Armstrong,  
P. H. Thomas,

Thos. L. Carthy,  
Levin B. Lane.

*South Carolina.*

Thomas Smith,

Thos. A. Mc Iver,

S. Blanding.

*Ohio*—Jeremiah Brooks.

*Georgia.*

Cullen Lockett,  
Alexander Jones,  
A. F. Hill,  
John Jordan,

Richard Banks,  
Joel Branham,  
J. D. Swift,

J. D. Jarrett,  
Willis Greene,  
Wm. Raney.

## Lectures delivered during the last Session.

1820, *Nov. 11th.* Dr. Wm. P. Dewees—"The mode of arresting the hæmorrhage attending certain cases of abortion."

*Nov. 18th.* Dr. Rousseau—"The circulation of the Blood."

*Nov. 24th.* Dr. Atlee—"The Ergot."

*Dec. 2d.* Dr. Barnes—"The importance of the Skin in Pathology."

*Dec. 9th.* Dr. T. M. Hall—"Marasmus."

*Dec. 16th.* Dr. Eberle—"Animal Life."

*Dec. 23d.* Dr. Shoemaker—"The effect of certain Moral and Physical causes over the act of Parturition."

1821, *Jan. 6th.* Dr. T. D. Mitchell—"Febrile Contagion."

*Jan. 13th.* Dr. Wood—"Congestion."

*Jan. 27th.* Dr. Hare—"Galvanism."

Dr. Horner—"The Functions of the Brain and Medulla Spinalis."

*Feb. 3d.* Dr. Bache—"Some of the more important applications of Chemistry to Medicine."

*Feb. 10th.* Mr. Wm. Lee—"Strictures of Urethra and Fistula in Perineo."

## List of Lecturers for Session, 1821-22.

Drs. Barnes, Harris, Dewees, Parrish, Atlee, Colhoun, Eberle, W. P. C. Barton, Horner, Hare, J. G. Nancrede, Mitchell, Bache, Stewart, Jackson, Cleaver, Povall, Price, Emlen, Shoemaker, Lawrence, Rousseau, R. M. Patterson, Meigs, Hartshorne.

JAMES M. STAUGHTON, *Sec'y.*

Annual Medical Commencement in the College of Physicians and Surgeons of the University of the state of New York, April 3, 1821.

Under the authority of the Honourable the Regents of the University, the degree of Doctor of Medicine was conferred by the Vice President, Dr. Post, on the following gentlemen :

Solomon A. Arnold, of Rhode Island, *on the Pestis Tropicus.*

Gerard Bancker, of New York, on *Diseases of the Liver*.  
 Hersey Baylies, of New York, on *Lithotomy*.  
 William A. L. Collins, of Georgia, on *Aneurism*.  
 James Demarest, of New Jersey, on *Hydrocephalus*.  
 Thomas J. Epps, of Virginia, on *Anasarca*.  
 Ralph C. Elliot, A. B. of South Carolina, on *Cold Bathing*.  
 Edward H. Fisher, of South Carolina, on *Dropsy*.  
 Wilson Faulke, of Tennessee, on *Diseases of the Liver*.  
 Robert L. Green, A. B. of South Carolina, on *Syphilis*.  
 Robert Greenhow, of Virginia, on *Galvanism*.  
 Lana J. Hancock, of South Carolina, on *Influenza*.  
 William Hume, of South Carolina, on the *Functions of the Lungs*.  
 William Ley, A. B. of South Carolina, on *Phthisis Pulmonalis*.  
 Thomas L. Lamar, of Georgia, on *Uterine Hemorrhage*.  
 Lueco Mitchell, of North Carolina, on *Dyspepsia*.  
 Elijah Mead, of Massachusetts, on the *Spirea Tomentosa*.  
 Alexander M. Montgomery, of New Jersey, on *Scurvy*.  
 James McFarlane, of South Carolina, on the *Fourth Stage of Labour*.  
 John Neilson, junr. A. B. of New York, on *Mania*.  
 Richard Pennell, of New York, on the *Bilious Remittent Fever*.  
 J. Smyth Rogers, A. B. of New York, on *Dyspepsia*.  
 Jacob Schmidt, of South Carolina, on *Sulphur Sublimatum*.  
 Jacob I. Swann, of Virginia, on the *Diseases of Females*.  
 Samuel T. Treat, of New York, on *Cruritis*.  
 Henry A. Tatum, of Virginia, on *Dysentery*.  
 John Allen Taylor, of New Jersey, on *Trachitis*.  
 Abraham D. Wilson, A. B. of New York, on *Hydrocephalus*.  
 Robert C. Wood, of Rhode Island, on *Hereditary Diseases*.  
 Reuben C. Worthington, A. B. of South Carolina, on *Cantharides*.

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 UNIVERSITY OF PENNSYLVANIA.

At a public commencement, held on the fifth of April, 1821, the following gentlemen received the degree of *Doctor of Medicine*, viz.

*New Hampshire.*

Jonas Underwood, on *Cutaneous Sympathy*.

*Connecticut.*

George Marvin, *Periodic Headach*.

*New York.*

Francis S. Beattie, *Tetanus*.

Theophilus C. Dunn, *Tetanus*.

*New Jersey.*

George Hains, *Cataract*.

Okey H. Costill, *Chlorosis*.

Thomas Page, *Dysentery*.

Henry Holcombe, *Marasmus*.

Thos. Jones Van Meter, *Canker*.

John Brick, *Traumatic Hæmorrhage*.

*Pennsylvania.*

James M. Staughton, *Mania à potu*.

John Russell, *Dysentery*.

Benjamin Rush Rees, *Yellow Fever*.

James R. Speer, *Bilious Colic*.

Thomas McEuen, *Injuries of the Head*.

Ezekiel C. Cook, *Cholera Infantum*.

Edwin P. Atlee, *Dysentery*.

Samuel Freedley, *Hepatitis*.

Jacob Hittel, *Dropsy*.

Richard Wilson, *Tic Douloureux*.

Joseph Griffith, *Dysentery*.

James Hunter Ewing, *Amaurosis*.



*Maryland.*

William Plater, Phlegmasia dolens.  
 Stephen White, Dysentery.  
 William L. Jones, Epilepsy.

William Gray, Phlegmasia dolens.  
 Christopher Stoddert, Dysentery.  
 James A. Shorb, Menstruation.

*Virginia.*

Rich. J. M'Kim Holiday, Fungus of  
 Antrum Maxillaire.  
 Richard Short, Cholera Infantum.  
 John Hendree, Puerperal Fever.  
 William M. Maxwell, Gastritis.  
 Charles C. Byrd, Secale Cornutum.  
 Charles Sturdivant, Amenorrhœa.  
 Robt. E. Taylor, Cynanche Trachealis.  
 Raleigh T. Colston, Menstruation.  
 Edward P. Scott, Menstruation.  
 Isaac Telfair, Hæmorrhoids.  
 Landon C. Rives, Oil of Turpentine as  
 a remedy.  
 L. G. Coleman, Cynanche Trachealis.  
 Jones Oliver Christian, Dysentery.

James Somerville, Experiments on  
 Absorption.  
 Samuel C. Snyder, Cynanche Trache-  
 alis.  
 Horace W. Bramham, Dyspepsia.  
 Henry E. Shore, Dysentery.  
 Thomas Old, Dysentery.  
 Wyatt Christian, Peritonitis.  
 George Claiborne, Bilious Disease of  
 King William County.  
 Richard W. Royster, Leucorrhœa.  
 Theodorick A. Carter, Atrophia Ab-  
 lactatorum.  
 William Stith, Digestion.  
 Robert H. Cabell, Pleurisy.

*North Carolina.*

Josiah C. Skinner, Hydrothorax.  
 L. H. Coleman, Typhus.  
 Isaac N. Jones, Typhus.  
 William W. Stewart, Pneumonia Ty-  
 phoides.

John S. Jackson, Schirro-Cancer of  
 Uterus.  
 Isaac Pipkin, Phthisis Pulmonalis.

*South Carolina.*

Edward Cuthbert, Hysteritis.  
 William S. Price, Dysentery.

Thomas Smith, Enlargement of Pros-  
 tate Gland.  
 William F. Lee, Dyspepsia.

*Georgia.*

Richard Banks, Hydrocele.

Cullen Lockett, Hepatitis.

*Tennessee.*

William W. Lea, On Fever.

*Missouri.*

George P. Todson, Chorea Santi Viti.

At the same time, the following gentlemen received the degree of *Master of Pharmacy*.

John Hart, No. 8, South Second street.  
 John White, No. 82, North Front street.  
 John Y. Bryant, No. 27, Market street.  
 Robert Milnor, No. 161, South Second street.  
 Anthony Ecky, No. 53, South Seventh street.

Thomas Cave, No. 45, Market street.  
 Charles Marshall, jr. No. 310, Market street.  
 Christopher Marshall, No. 19, North Fourth street.  
 Mordecai Y. Bryant, No. 241, North Second street.  
 Mordecai L. Gordon, No. 58, North Second street.  
 James D. Rooney, S. W. corner Green and Second streets.  
 Charles Treichel, No. 99, Walnut street.  
 George F. Garretson, corner Race and Fourth streets.  
 David B. Ayers, No. 164, North Third street.  
 John Stitt, corner Race and Sixth streets.  
 William Poole, No. 62, North Second street.

J. R. COXE, *Dean of the Faculty.*

#### UNIVERSITY OF MARYLAND.

The following Graduates received the Degree of Doctor of Physic in this Institution, April 2, 1821.

##### *Virginia.*

\* Ed. C. Alexander, De Dysenteria. John Morgan, Fever.

##### *Maryland.*

Lloyd Dorsey, Digestion.	George A. Barber, Pneumonia.
Ephraim Bell, Typhus.	Samuel K. Handy, Croup.
Abraham Street, Physiolsy.	Francis Neale, Hydrocep. Inter.
John P. McKenzie, Meloe Vesii.	Benjamin Day, Gastritis.
Abraham Jessop, Hæmoptysis.	Joseph Elbert, Apoplexy.
Richard W. Davis, Typhus.	William Hammond, Cyna. Trach.
William M. B. Wilson, Dyspepsia.	Richard G. Belt, Amenorrhœa.
Thomas B. Magruder, Dysentery.	Parker Forwood, Tetanus.

##### *Delaware.*

Jacob Fisher, Dysmenorrhœa. Edward K. Huffington.

*Pennsylvania*—John W. Davis, Gunshot Wounds.

*North Carolina*—Lawrence O'Bryan.

*Missouri*—Leo Fenwick.

*Louisiana*—Charles G. Lewis—Rheumatismus.

#### *The Degree of Bachelor of Physic.*

William Morgan, Delaware; Augustine Taney, Maryland; John Swope, Maryland; John Daily, Maryland.

JNO. B. DAVIDGE, *Dean of the Faculty.*

#### LITERARY NOTICES.

(*From the British Medical Journals of January 1821.*)

In the press, a new (being the third) edition of Dr. Johnson's work on Tropical Climates. The diseases of the *Western* hemisphere will be investigated

\* Dr. Alexander obtained the Prize for the *Latin Thesis.*



with great minuteness, and all the light which the last twenty years have elicited respecting them, will be collected into a focus in this edition. A physician of great talents, and long resident in the West Indies, is assisting the author in arranging and enriching this enlarged division of the Work. Great and important additions will be made to the *Eastern* division, and the whole Work is undergoing a thorough revision. It will be published in February, 1821.

In the Press, in one very small volume, "Observations on the Climate of Penzance, and District of the Land's End in Cornwall," &c. By Dr. John Forbes, of Penzance.

In the Press, and will appear in January, a Treatise on the Epidemic Cholera of India. By James Boyle, Surgeon to his Majesty's ship *Minden*.

Professor Robbi, of Leipsic, has lately published a German Translation of Mr. Curtis's Treatise on the Physiology and Diseases of the Ear. The subject appears to be entirely new in Germany; and the Professor has enriched his Translation with many valuable notes highly complimentary to the Author, and strongly recommends to his countrymen an institution similar to the Royal Dispensary for curing Diseases of the Ear in this country; to his Translation he has prefixed Mr. Curtis's Original Plate of Acoustic Instruments.

In the Press, and speedily will be published, the Principles of Forensic Medicine. By J. D. Smith, M. D.

Mr. S. F. Gray has in the Press, and nearly ready for publication, a new and greatly improved Edition of his Supplement to the Pharmacopœias.

A new and enlarged Edition of Mr. A. Thomson's Conspectus of the London, Edinburgh, and Dublin Pharmacopœias, will be published in January.

Dr. Ramsbottom has nearly ready for publication, in one volume 8vo. Practical Observations in Midwifery; with a Selection of Cases.

On the 31st of January, 1821, will be published, No. 1, and continued monthly, of the British Domestic Herbal; being a correct Description of British Medicinal Plants. Intended for the Use of Families, and for every Purpose of Domestic Medicine. Illustrated by Plants, accurately coloured according to Nature.

Preparing for publication, (Second Edition), an Analysis of the Medicinal Waters of Llandeindod, in Radnorshire; with Observations upon the Diseases to which they are applicable, and Directions for their Use. To which is prefixed, a Topographical Account of the Place. By Richard Williams, Surgeon at Aberystwyth, Licentiate of the Society of Apothecaries, Honorary Member of the Physical Society of Guy's Hospital, &c. &c. &c.

The Proprietor of the American Medical Recorder, is about to put to Press the following Books, viz.

Cooper's Dictionary of Practical Surgery, from the last revised, corrected, and enlarged London edition, with notes and additions, by an eminent Practitioner, embracing many important improvements in Surgery, which have been made since Mr. Cooper's last edition. The Pharmacologia, or the language employed in expressing the remedies, will be accommodated to the American Pharmacopœia.

A Treatise on Derangements of the Liver, Internal Organs, and Nervous System. By James Johnson, M. D. Author of the "Influence of Tropical Climates on European Constitutions," &c. &c. from the last London edition, revised and improved.

Elements of Pathology and Therapeutics, being the outlines of a work intended to ascertain the nature, causes, and most efficacious modes of Prevention and Cure, of the greater number of the Diseases incidental to the Human Frame; illustrated by numerous Cases and Dissections. By Caleb Hillier Parry, M. D. F. R. S. member of the College of Physicians of London, &c. &c. &c. from the last London edition, in two volumes.

**Surgical Essays.** By Astley Cooper, F. R. S. Surgeon to Guy's Hospital; and Benjamin Travers, F. R. S. Surgeon to St. Thomas's Hospital, in two parts, from the last London edition.

**On the Nature, Causes, and Varieties of the Arterial Pulse.** By C. H. Parry M. D. F. R. S. &c. &c. &c.

In Press, and will shortly be published by Edward Parker, No. 178, Market street, Philadelphia, A Treatise on Ruptures, containing an Anatomical Description of each species; with an account of its symptoms, progress, and treatment. By William Lawrence, F. R. S. Professor of Anatomy and Surgery to the Royal College of Surgeons, &c. &c. &c. from the third London edition, revised, corrected, and enlarged.

#### A LIST OF LATE BRITISH MEDICAL PUBLICATIONS.

- Allan's (R.) System of Pathology and Operative Surgery founded on Anatomy, vol. 1, 8vo.
- Anatomical Examinations. A complete Series of Anatomical Questions and Answers. The Answers arranged so as to form an Elementary System of Anatomy, and intended as preparatory to Examination at Surgeon's Hall, &c. 2 vols. 12mo.
- Armstrong's (Dr. John) Facts and Observations relative to the Fever commonly called Puerperal, second edition, 8vo.
- Armstrong's (Dr. John) Practical Illustrations of the Scarlet Fever, of Measles, of Pulmonary Consumption, and of Chronic Nervous Diseases, second edition, 8vo.
- Ayre's (Dr. Joseph) Practical Observations on the Nature and Treatment of Marasmus, 8vo.
- Bell's (Charles) Treatise on the Diseases of the Urethra, Prostate Gland, Vesica Urinaria, and Rectum, reduced into Systematic Order, with Notes, &c. By John Shaw, 8vo.
- Blumenbach's (J. F.) Institutions of Physiology, translated by J. Elliotson, M. D. third edition, 8vo.
- Burns' Principles of Midwifery; including the Diseases of Women and Children, fifth edition, 8vo.
- Burrows (Dr. G. M.) on Errors relative to Insanity, 8vo.
- Clark's (Dr. J.) Medical Notes on Climate, Diseases, Hospitals and Medical Schools in France, Italy, &c. 8vo.
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